







A  
JOURNAL  
OF  
NATURAL PHILOSOPHY,  
*CHEMISTRY,*  
AND  
THE ARTS.

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VOL. VI.

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*Illustrated with Engravings:*

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BY WILLIAM NICHOLSON.

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LONDON:

PRINTED BY W. STRATFORD, CROWN-COURT TEMPLE-BAR FOR  
THE AUTHOR, No. 10, SOHO-SQUARE:

AND SOLD BY

G. AND J. ROBINSONS, PATERNOSTER-ROW.

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## ADVERTISEMENT.

**T**HE Authors of Original Papers in the present Volume, are Mr. Frederick Accum; Mr. Irwine; Sir. A. N. Edelcrantz; Dr. T. C. Hope, F. R. S. Edinburgh; Anthony Carlisle, Esq.; M. B. Donkin; Mr. Ezekiel Walker; Thomas Thompson, M. D.; Dr. Bostock; Mr. Dalton; Edward Howard, Esq. F. R. S.; Mr. A. Woolf; Andrew Duncan, M. D. F. R. S. Edinburgh; Dr. Prince; Mr. J. C. Hornblower, and Mr. G. Smart. Of Foreign Works, M. Regnier; Wiegleb; Guyton; Curaudau; Baunach; Payffé; Parmentier; Steinacher; Schaub; Nicolai; Bouillon la Grange; Klaproth; Lomet; Tromsdorff; Seguin; Ritter; Thenard; Vauquelin; La Place; Guyton-Morveau; Von Hombolt; Raymond; Berthollet, and Hassenfratz. And of English Memoirs abridged or extracted, Wm. Herschell, L. L. D. F. R. S.; Richard Chenevix, Esq. F. R. S. and M. R. I. A.; Humphry Davy, Esq.; Wm. Hyde Wollaston, M. D. F. R. S.; Sir H. C. Englefield, Bart. F. R. S.; James Smithson, Esq. F. R. S. P. R. I.; Mr. James Woart; William Fairman, Esq.; Charles Hatchett, Esq. F. R. S.; The Right Hon. C. Greville, F. R. S.; Mr. John Dalton; Everard Home, Esq. F. R. S.; and Andrew Duncan, Junr. M. D. F. R. S. Edinburgh.

Of the Engravings the Subjects are, 1. Dr. Young's Apparatus for illustrating the Doctrine of Preponderance. 2. Apparatus for Experiments with Spouting-Fluids. 3. A Lock of Combination by M. Regnier. 4. Solar Phenomenon observed, by Sir H. Englefield, Bart. 5. Figure to shew the Proportion of the magnified Images of the same Star at different Times, by Dr. Herschell. 6. Sir A. N. Edelcrantz's Method of Raising Water in Worm-Tubs, Condensers, &c. 7. Eudiometric Apparatus, by Dr. Hope. 8. Vessel for In-

closing

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*Soho Square, December, 1803.*

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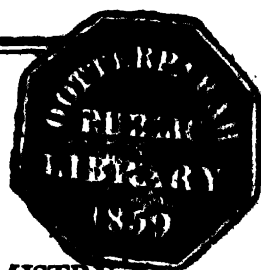
# JOURNAL

OF

## NATURAL PHILOSOPHY, CHEMISTRY,

AND

## THE ARTS.




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SEPTEMBER, 1803.

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### ARTICLE I.

*Experiments and Observations on the Compound of Sulphur and Phosphorus, and the dangerous Explosions it makes when exposed to Heat. By FREDERICK ACCUM, Practical Chemist and Teacher of Chemistry. Communicated by the Author.*

MARGRAFF, I believe, was the first who noticed the Compound of combination of phosphorus and sulphur; and Pelletier examined afterwards this compound, and pointed out some of its properties\*. The latter philosopher showed, at least, that the compound resulting from the union of phosphorus and sulphur, in different proportions, is infinitely more fusible than either of them taken separately. Repeating the experiments of the French philosopher, I had no apprehension that the combination of these two simple bodies was attended, under certain circumstances, with consequences which might prove fatal to the chemical operator. And it is with the view of preventing my brother chemists from falling a sacrifice to unexpected dangers, that I shall relate an accident, which might have been attended with the most dangerous consequences, before I state the properties which characterise the compound which is the subject of these lines. The ac-

*Compound of phosphorus and sulphur; when first examined, not supposed to be attended with danger.*

\* Journal de Physique, xxxv. 383.



Very dangerous explosion from the combination of half an ounce of phosphorus, six ounces of water and one ounce of sulphur on a sand heat.

cident alluded to, happened in the following manner: Half an ounce of phosphorus, cut into pieces of the size of a pea, was introduced into a Florence flask, containing about ten ounces of water; one ounce of sulphur broken into fragments of about the same size was added, and the whole placed on a heated sand-bath. In a few minutes the union of the phosphorus and sulphur was effected. On leaving the whole in the heated sand, for about ten minutes longer, the empty part of the flask became filled with dense white fumes, which increased more and more; being unable to observe what change was taking place, I carefully removed the flask out of the sand-bath, and agitated the fluid in such a manner, that the fused compound of phosphorus and sulphur still remained under the surface of the water. But the instant this was done, the whole exploded in my hand with a tremendous report; the mixture of the burning phosphorus was thrown into my face, and occasioned very painful wounds; the pieces of the Florence flask were scattered all over the laboratory, as fine as sand, and the larger parts of the neck of this vessel were driven into my right hand, as well as into the wall, to a considerable depth.

Repetition of the experiment with smaller and also with larger quantities of the materials. Explosion as before.

Anxious to understand the nature of this unexpected explosion I again exposed to heat, in a similar manner, two drachms of phosphorus and half an ounce of coarsely powdered sulphur, in a small flask containing four ounces of water. The mixture, after having been left in a heated sand-bath for about ten minutes, exploded with prodigious violence, and a flash of fire rose up to the ceiling. The same experiment was repeated with larger quantities of phosphorus and sulphur, three successive times, with similar effects. These experiments were made at the laboratory, and in the presence of the Right Honourable Lord Camelford, who liberally supplied the materials for these and the following experiments, and permitted them to be made on his premises.

Another accidental explosion of the same nature.

Before I advance any thing further concerning the accension of this compound, I beg leave to relate one instance more of a similar nature, which happened lately in my own laboratory. Mr. Garden, a philosophical gentleman immersed into a vessel filled with warm water, a vial containing six ounces of phosphorus, to which had been previously added one drachm of a mixture of phosphorus and sulphur. The contents of the

the vial being liquified, (which was the intent of immersing it into heated water) he removed the vial out of the fluid, taking care to close its orifice with his finger, and then agitated it gently. The moment this was began the vial burst to pieces with a report like a gun, the burning mixture was thrown in all directions, and the whole laboratory was filled for some hours with a very dense cloud of white vapours.

Being thus sufficiently convinced of the danger which attends the combination of phosphorus with sulphur, under such circumstances, I introduced into a Wedgwood's tube closed at one end, two drachms of phosphorus, and double that quantity of sulphur. I then added four ounces of water, and closed the other extremity of the tube with a cork, into which a bended tube was cemented, which terminated under a glass cylinder filled with mercury, standing inverted in a basin containing the same fluid. I then rectified the tube, and applied heat to that part which contained the phosphorus and sulphur; on increasing the heat gradually a quantity of gas was collected, which amounted to nearly two quarts. But no explosion took place.

Careful examination of the phenomena, Phosphorus, sulphur and water exposed to distillation.

afforded a gas without explosion.

To learn the nature of this gas, I transferred a quantity of it into the water apparatus, and agitated it in contact with that fluid strongly for a few minutes. Its volume was now considerably diminished. On repeating the experiment in distilled water, it was found that this fluid absorbed nearly  $\frac{1}{2}$  of its own bulk. On sending up one part of atmospheric air into a cylinder holding six parts of this gas, an instantaneous inflammation ensued, the cylinder became filled with white fumes, and a white crust lined the inner surface of the glass. Finding thus that the gaseous product was decomposable by atmospheric air, I collected another quantity of gas, in a similar manner as before; and mingled it gradually with oxygen gas till no further accension ensued. The gas left behind amounted to  $\frac{1}{24}$  of the whole. It had all the properties of nitrogen gas.

which, added to common air, diminished that fluid;

but took fire when a small portion of common air was added to a larger of the gas.

The residue of this gas decomposed by oxygen was azote.

The white flakes which were collected from the sides of the glass cylinder, as well as from the surface of the mercury over which the experiments were made, attracted moisture rapidly, and became converted into a cream-like fluid. They consisted of sulphur, sulphuric and phosphoric acids.

The phosphates were sulphur, sulphuric and phosphoric acids.

Oxygenized muriatic acid gas acted more violently than oxygen gas, when mingled with this gaseous compound over mercury; the result was a considerable detonation, accompanied with vivid green light and dense white vapours.

The gas was therefore a compound of hydrogen, sulphur, and phosphorus; of the production of this gas, it is evident, that, during the action of the phosphorus and sulphur upon water, the latter fluid is decomposed, though neither sulphur nor phosphorus singly taken, can effect this decomposition; this therefore is sufficient to account for the unexpected explosion before stated.

formed by the decomposition of water; though that fluid is not affected by either singly.  
Phosphuret of sulphur can decompose water at the common temperature.

In order to see whether phosphuret of sulphur were capable of decomposing water in common temperatures, two ounces of it were covered in a vial with eight ounces of water, and put aside for further examination. The vial having been left unobserved, locked up in a closet, for some weeks; the corks was found to have been thrown out of the vial, and the whole inside of the closet, which had been painted with white lead, was completely blackened; the parts nearest to the orifice of the vial had a metallic aspect. The fluid which was decanted from the phosphuret of sulphur had a milky appearance, its odour was like that of water strongly impregnated with sulphurated hydrogen; its taste was uncommonly nauseous. It had a strong action on the greater number of metallic oxides.

On mingling it with concentrated nitrous acid, a considerable precipitate ensued, which after being dried on exposure to air, was luminous in the dark and became converted into sulphur. Another quantity of the water which was suffered to evaporate, spontaneously deposited crystals of a lemon yellow colour, but of an indeterminate figure. There remained, therefore, no doubt but that phosphuret of sulphur is capable of decomposing water at usual temperatures.

As phosphuret of sulphur decomposes air with greater rapidity than any other substance, it will be a good eudiometrical agent.

Phosphuret of sulphur, composed of three parts of phosphorus and one of sulphur, has also the property of decomposing atmospheric air with great rapidity. It may, therefore, be employed more advantageously for eudiometrical processes than either phosphorus, or the sulphurets of earths, alkalies, & metals. If into a dry glass tube closed at the top, and graduated

graduated into equi-distant parts, a quantity of phosphuret of sulphur, freed from adhering water, be poured, and agitated in the tube, so as to line a considerable part of it within, a vast quantity of white vapour is produced the moment the tube is immersed in water; so as to exclude the air. The vapours will be absorbed by the water, and when no further clouds appear, the process is at an end. The residuary gas will then be found to be the quantity of nitrogen gas, which was contained in the air experimented upon in the tube. This process is far more expeditious than the slow combustion of phosphorus. I must, nevertheless, remark, that phosphuret of sulphur *like all other substances* hitherto employed for eudiometry, cannot be absolutely depended upon for ascertaining the absolute quantity of oxygen, contained in a given portion of atmospheric air. For as soon as the absorption of oxygen is completed, the remaining nitrogen exercises an action upon the phosphorus, by means of which its bulk becomes increased. From a number of experiments made with that view with this eudiometrical substance, I am led to believe that the volume of nitrogen gas, never increases so much as to  $\frac{1}{3}$  part; consequently the bulk of the residuum, diminished by  $\frac{1}{3}$  gives us the bulk of the nitrogen gas of the air examined; which bulk subtracted from the original mass of air, indicates the proportion of oxygen gas contained in it.

This phosphuret however, has the usual imperfection of changing the residue.

The change is small and may be allowed for.

Phosphuret of sulphur of the above composition also decomposes nitrous acid with uncommon rapidity at common temperatures. If one part of phosphuret of sulphur be introduced in the cold into four or six of concentrated nitrous acid, a violent action takes place, the acid is decomposed, and both the phosphorus and sulphur are oxygenized, at the expense of the oxygen of the nitric acid. A clear solution is obtained, from which phosphoric and sulphuric acid may be separated in the usual manner.

This phosphuret decomposes nitrous acid very rapidly.

Phosphuret of sulphur is soluble in expressed or fat oils. If one part of this compound, freed from adhering moisture as much as possible, be triturated in a Wedgwood's mortar, with six parts of oil of almonds or olives, a liquid phosphorus is obtained, which is far superior to that produced in the usual manner from mere phosphorus. This liquid phosphorus shines and gives a very fine liquid phosphorus, with a beautiful yellow light. It may be rubbed over the face, hands, &c. without injury,

If Phosphuret of sulphur is soluble in fat oils.

injury, provided the fluid be perfectly transparent, and consequently contains not a particle of phosphuret of sulphur mechanically suspended.

Which is uncommonly luminous.

The luminous property of this liquid phosphorus is so considerable, that about 4oz. of it, when contained in a common size wine decanter, gives a sufficient light to discern objects at a considerable distance in a large room, the moment the decanter is unstopped.

Beautiful experiment of a luminous shower.

Equal parts of this liquid phosphorus and oil of turpentine, when agitated together and poured out of any convenient perforated vessel, exhibits a beautiful phenomenon, greatly resembling a luminous rain, or shower of fire.

Caution against heat.

Though a liquid phosphorus may be obtained, as directed before, no attempt should be made to apply heat to a mixture of phosphuret of sulphur. For an explosion was always the result whenever I attempted any process of that kind.

Phosphuret of sulphur is soluble in ether.

Phosphuret of sulphur is soluble in sulphuric and nitric ether. If either of these fluids be suffered to stand for some weeks, over a quantity of phosphuret of sulphur in a closed vial; part of the compound becomes dissolved in the ether. If the ether be suffered to evaporate spontaneously, or when assisted by heat, a multitude of exceedingly small crystals are left behind, which shine in the dark with a brilliant yellow light. A piece of cloth dipped into this ether appears luminous in the dark all over, but in a few minutes this luminous appearance ceases, and the whole appears to be sprinkled over with gems. If a feather, or piece of tow, be dipt in water, and then thrown into a bottle containing ether impregnated with phosphuret of sulphur; at the moment of contact of the two fluids, a sudden light of a yellow colour spreads through the air, undulates along the surface of the fluid, and illuminates the whole bottle.

Phosphuret of sulphur in oil of turpentine, and other volatile oils.

Phosphuret of sulphur is soluble in pure or rectified oil of turpentine, oil of rosemary, oil of lavender, and in the rest of the volatile oils met with in commerce. The solutions are all luminous in the dark, and deposit the phosphuret of sulphur when slowly evaporated, in the form of needle-shaped crystals. Highly rectified alcohol takes up a small quantity of phosphuret of sulphur, the alcoholic solution is decomposable by the addition of water.

Sparsingly in alcohol.

It takes fire in ox. mur. acid gas.

Phosphuret of sulphur takes fire spontaneously in oxygenized muriatic acid gas. If a small quantity of dry phosphuret of sulphur

phur be introduced into a metallic spoon, and then immersed in a bottle filled with oxygenized muriatic acid gas; the compound instantly kindles and burns with great vividness. The results of this experiment of course are phosphoric, sulphuric, and muriatic acids.

Phosphuret of sulphur, when in a state of inflammation burns also in nitrous gas, and in gaseous oxide of nitrogen.

If a piece of cotton be impregnated with phosphuret of sulphur, and then surrounded with wool or tow, and placed under the receiver of an air-pump, the compound shines with a beautiful yellow light, which increases in proportion as the air is more rarefied. On re-admitting a small portion of air, a beautiful *Corona* or *Aurora Borealis* pervades the receiver. If a thermometer be included into the cotton containing the phosphuret of sulphur, it rises in proportion as the light increases, and in this respect, as well as in the former, the phosphuret of sulphur answers better for this experiment, which was first noticed by Van Marum, who made use of phosphorus.

The formation of phosphuret of sulphur seems not to be attended with any change of temperature, as is said to be always the case in all chemical combinations whatever. For if the phosphorus and sulphur be immersed in heated water, at a distance from each other, together with a thermometer, no increase or decrease of temperature could be observed by means of the most delicate instrument. The compound of phosphorus and sulphur acts more violently in destroying animal life, than phosphorus alone. A cat which had eaten two grains of phosphorus repeatedly without impunity, died within half an hour after having swallowed one grain of phosphuret of sulphur.

Old Compton-Street, Soho,

August 16, 1803.

## II.

(Concluded from Page 304. Vol. V)

*Observations of the Transit of Mercury over the Disk of the Sun ; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By WILLIAM HERSCHEL, LL. D. F. R. S.*

Mercury neatly defined.

WITH a 10-foot reflector, and magnifying power of 130, I saw the corrugations of the luminous solar surface, up to the very edge of the whole periphery of the disk of Mercury.

10<sup>h</sup> 27'. When the planet was sufficiently advanced towards the largest opening of the northern zone, I compared the intensity of the blackness of the two objects ; and found the disk of Mercury considerably darker, and of a more uniform black tint, than the area of the large opening.

10<sup>h</sup> 32'. The preceding limb of Mercury cuts the luminous solar clouds with the most perfect sharpness ; whereas, in the great opening, the descending parapet, down the preceding side, was plainly visible.

It should be remarked, that the instrument here applied to the sun, with the moderate power of 130, is the same 10-foot reflector which, in fine nights, when directed to very minute double stars, will show them distinctly with a magnifier of 1000.

Great magnifying power not suitable to the sun.

Having often attempted to use high magnifiers in viewing the sun, I wished to make another trial ; though pretty well assured I should not succeed, for reasons which will appear hereafter.

With two small double convex lenses, both made of dark green glass, and one of them having the side which is nearest the eye thinly smoked, in order to take off some light, I viewed the sun. Their magnifying power was about 300 ; and I saw Mercury very well defined ; but that complete distinctness, which enables us to judge with confidence of the condition of the object in view, was wanting.

With a single eye-glass, smoked on the side towards the eye, and magnifying 460 times, I also saw Mercury pretty well defined ; but here the sun appeared ruddy, and no very minute objects could be perceived.

## TRANSIT OF MERCURY.

11<sup>h</sup> 28'. The planet having advanced towards the pre- Observations of  
ceding limb of the sun, it was now time to attend to the ap- the contacts.  
pearances of the interior and exterior contacts.

11<sup>h</sup> 32'. 10-foot reflector. The whole disk of Mercury is as Not the least  
sharply defined as possible; there is not the least appearance sign of any  
of any atmospheric ring, or different tinge of light, visible about Mercury.  
about the planet.

11<sup>h</sup> 37'. Appearances remain exactly as before.

11<sup>h</sup> 42'. The sharp termination of the whole mercurial disk,  
appears to be even more striking than before. This may be  
owing to its contrast with the bright limb of the sun, which,  
having many luminous ridges in the northern zone, is remark-  
ably brilliant about the place of the planet.

11<sup>h</sup> 44'. I was a few moments longer writing down the At the interior  
above than I should have been, to see the interior contact so contact, no dis-  
completely as I could have wished; however, the thread of tortion either of  
light on the sun's limb was but just breaking, or broken; but the sun's limb  
or the disk of  
Mercury,  
no kind of distortion, either of the limb or of the disk of Mer-  
cury, took place.

The appearance of the planet, during the whole time of its nor during the  
emerging from the sun, remained well defined, to the very emerging nor  
last. afterwards.

The following limb of Mercury remained sharp till it reached  
the very edge of the sun's disk; and vanished without occa-  
sioning the smallest distortion of the sun's limb, in going off,  
or suffering the least alteration in its own figure.

As soon as the planet had quitted the sun, the usual appear-  
ance of its limb was so instantly and perfectly restored, that not  
the least trace remained whereby the place of its disappearance  
could have been distinguished from any other adjacent part of  
the solar disk.

It will not be amiss to add, that very often, during the transit, No signs of  
I examined the appearance of Mercury with a view to its an oblate figure.  
figure, but could not perceive the least deviation from a spher-  
ical form; so that, unless its polar axis should have happened  
to be situated, at the time of observation, in a line drawn from  
the eye to the sun, the planet cannot be materially flattened at  
its poles.



OBSERVATIONS AND EXPERIMENTS RELATING TO THE  
CAUSES WHICH OFTEN AFFECT MIRRORS, SO AS TO  
PREVENT THEIR SHOWING OBJECTS DISTINCTLY.

The action, of  
reflecting tele-  
scopes is very  
different at dif-  
ferent times.

It is well known to astronomers, that telescopes will act very differently at different times. The cause of the many disappointments they may have met with in their observations, is however not so well understood.

Sometimes we have seen the failure ascribed to certain tremors, as belonging to specula; and remedies have been pointed out for preventing them. Not unfrequently again, the telescope itself has been condemned; or, if its goodness could not admit of a doubt, the weather in general has been declared bad, though possibly it might be as proper for distinct vision as any we can expect in this changeable climate.

The experience acquired by many years of observation, will however, I believe, enable me now to assign the principal cause of the disappointments to which we are so often exposed. Unwilling to hazard any opinion that is not properly supported by facts, I shall have recourse to a collection of occasional observations. They have been made with specula of undoubted goodness, so that every cause which impeded their proper action must be looked upon as extrinsic. I shall arrange these observations under different heads, that, when they have been related, there may remain no difficulty to draw a few general conclusions from them, which will be found to throw a considerable light upon our subject.

*Moisture in the Air.*

Whether mois-  
ture in the air  
impedes the  
action of tele-  
scopes.

(1.) October 5, 1781. I see double stars, with 460, completely well. The air is very damp.

(2.) Nov. 23, 1781. 15<sup>h</sup> 30'. The morning is uncommonly favourable, and I see the treble star  $\zeta$  Cancri, with 460, in high perfection. The air is very moist, and intermixed with passing clouds.

(3.) Sept. 7, 1782. I viewed the double star preceding 12 Camelopardalis,\* with 932. In this, and several other fine nights which I have lately had, the condensing moisture on the tube of my telescope has been running down in streams; which proves that damp air is no enemy to good vision.

\* See Phil. Transf. Vol. LXXV. Part I. page 68; II. 53.

(4.) Dec.

(4.) Dec. 28, 1782. 17<sup>h</sup> 30'. The water condensing on my tube keeps running down; yet I have seen very well all night. I was obliged to wipe the object-glasses of my finder almost continually. The specula, however, are not in the least affected with the damp. The ground was so wet that, in the morning, several people believed there had been much rain in the night, and were surprised when I assured them there had not been a drop.

(5.) Feb. 19, 1783. I have seen perfectly well till now \* that a frost is coming on; though Datchet Common, which is just before my garden, is all under water; and the grass on which I stand with my telescope is as wet as possible.

(6.) Feb. 26, 1783. All the ground is covered with snow; yet I see remarkable well.

(7.) March 8, 1783. The common before my garden is all under water; my telescope is running with condensed vapour; not a breath of air stirring. I never saw better.

(8.) August 25, 1783. My telescope ran with water all the night. The small speculum, which sometimes gathers moisture, was never affected in the 7-foot tube, but was a little so in the 20-foot. The large eye-glasses and object-glasses of the finders, required wiping very often. I saw all night remarkably well.

#### *Fogs.*

(9.) Oct. 30, 1779. It grows very foggy, and the moon is surrounded with strong nebulosity; nevertheless, the stars are very distinct, and the telescope will bear a considerable power. Whether fogs impair the distinctness of telescopes.

(10.) August 20, 1781. It is so foggy that I cannot see an object at the distance of 40 feet; yet the stars are very distinct in the telescope. By an increase of the fog,  $\alpha$  Piscium can no longer be seen by the eye; yet, in the telescope, it being double, I see both the stars with perfect distinctness.

(11.) Sept. 6, 1781. A fog is come on; yet I see very well.

(12.) Sept. 9, 1781. There is so strong a fog, that hardly a star less than 30° high is to be seen; and yet, in the telescope, at great elevations, I see extremely well.

\* The time is not marked in the journal; but, from the number of the observations that had been made during the night, it must have been towards morning.

(13.) March

(13.) March 9, 1783. It is very foggy; yet in the telescope I see the stars without aberration, and they are very bright.  $\alpha$  Serpentarii is without a single ray.

(14.) April 6, 1783. A very thick fog settles upon all my glasses; but the specula, even the 20-feet, which has so large a surface, remained untouched. I see perfectly well.

*Frost.*

Whether frost  
be an impediment  
to distinct  
vision by tele-  
scopes.

(15.) Nov. 15, 1780; five o'clock in the morning. An excellent speculum, No. 2, will not act properly; the frosty morning probably occasions an alteration in its figure. Another speculum, No. 1, acts but indifferently, though I have known it to shew very well formerly in a very hard frost: for instance, November 23, 1779, I saw with the same mirror, and a power of 460, the vacancy between the two stars of the double star Castor, without the least aberration.

(16.) Oct. 22, 1781. Frost seems to be no hindrance to perfect vision. The tube of my 7-feet telescope is covered with ice; yet I see very well.

(17.) Nov. 19, 1781. It freezes very hard, and the stars, even those which are  $50^{\circ}$  high, are very tremulous. I suspect their apparent diameters to be diminished; and, if I recollect right, this is not the first time that such a suspicion has occurred to me.

(18.) Jan. 10, 1782. My telescope would not act well, even at an altitude of 70 or 80 degrees. There is a strong frost.

(19.) Jan. 31, 1782. I cannot see with a power of 460, the stars seem to dance so unaccountably, and yet the air is perfectly calm: even at 60 or 70 degrees of altitude, vision is impaired.

(20.) Feb. 9, 1782. That frost is no hindrance to seeing well is evident; for, not only my breath freezes upon the side of the tube, but more than once have I found my feet fastened to the ground, when I have looked long at the same star.

(21.) Oct. 4, 1782. It froze very severely this night. At first, when the frost came on, I saw very badly, every object being tremulous; but, after some time, and at proper altitudes, I saw as well as ever. Between five and six o'clock in the morning, objects began to be tremulous again; occasioned, I suppose, by the coming on of a thaw.

(22.) Jan.

(22.) Jan. 1, 1783. I made a number of delicate observations this night, notwithstanding, at four o'clock in the morning, my ink was frozen in the room; and, at about five o'clock, a 20-foot speculum, in the tube, went off with a crack, and broke into two pieces. On looking at Fahrenheit's thermometer, I found it to stand at  $11^{\circ}$ .

(23.) May 6, 1783. It freezes, and in the telescope the stars seem to dance extremely.

#### *Hoar-frost.*

(24.) Nov. 6, 1782. There is a thick hoar-frost; yet I see extremely well. It seems to enlarge the diameters of the stars; but, as I see the minutest double stars well, the apparent enlargement of the diameters must be a deception. Hoar frost; its effect on telescopes.

(25.) Dec. 22, 1782. There is a strong hoar-frost gathering upon the tubes of my telescopes; but I see very well.

#### *Dry Air.*

(26.) Dec. 21, 1782. The tube of my telescope is dry, and I do not see well. Dry air inimical to distinct vision by telescopes.

(27.) April 30, 1783. The stars are extremely tremulous and confused; the outside of the tube of my telescope is quite dry.

#### *Northern Lights.*

(28.) Sept. 25, 1781. There are very strong northern lights; their flashing does not seem to interfere with telescopic vision; but all objects appear tremulous, and indifferently defined. Northern lights do not seem to impede vision by telescopes.

(29.) Aug. 30, 1782. There are very bright northern lights, in broad arches, with white streaks; yet I see perfectly well.

(30.) March 26, 1783. An Aurora Borealis is so bright, that  $\alpha$  Herculis, which it covers, can hardly be seen; yet, in the telescope, and with a power of 460, I find no difference. I compared the star with  $\gamma$  Coronæ, which was in a bright part of the heavens, and in the telescope they appeared nearly alike. I suspected  $\alpha$  Herculis to be somewhat more tinged with red than it should be; and examined it afterwards, when clear of the Aurora: it was indeed less red; but, as it had gained more altitude, the experiment was not decisive.

*Windy*

*Windy Weather.*

Wind impairs  
the effect of  
telescopes.

(31.) Jan. 8, 1783. It is very windy. The diameters of the stars are strangely increased, even those at 60 and 70° of altitude. Every star seems to be a little planet.

(32.) Jan. 9, 1783. Wind increases the apparent diameters of the stars.

(33.) Sept. 20, 1783. The night has been very windy ; and I do not remember ever to have seen so ill, with such a beautiful appearance of brilliant star-light.

*Fine in Appearance.*

Weather apparently fine but unfavourable.

(34.) May 28, 1781. The evening, though fine in appearance, is not favourable. No instrument I have will act properly. The wind is in the east.

(35.) August 30, 1781. The stars appear fine to the naked eye, so that I can see  $\epsilon$  Lyræ very distinctly to be two stars ; yet my telescope will show nothing well. There are flying clouds, which, by their rapid motion, indicate a disturbance in the upper regions of the air ; though, excepting now and then a few gusts of wind, it is in general very calm. At a distance there are continual flashes of lightning, but I can hardly hear any thunder.

(36.) Sept. 14, 1781. I see very small stars with the naked eye ; but the telescope will not act so well as it should.

(37.) Sept. 24, 1781. The evening is apparently fine ; but, with the telescope, I can see neither  $\alpha$  Coronæ nor  $\mu$  Bootis double ; nor indeed can I see any other stars well.

*Over a Building.*

Vicinity of a building renders the stars indistinct ;

(38.) August 24, 1783. I viewed  $\epsilon$  Bootis with 449,737, and 910, but saw it very indifferently. The star was over a house.

(39.) Oct. 26, 1780.  $\epsilon$  Bootis being near the roof of a house, I saw it not so distinctly as I could wish.

*The Telescope lately brought out.*

Recent exposure of the telescope does not afford distinctness.

(40.) Oct. 10, 1780. 6<sup>h</sup> 30'. Having but just brought out my telescope, it will not act well.

6<sup>h</sup> 45'. The tube and specula are now in order, and perform very well.

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TRANSIT OF MERCURY.

(41.) Jan. 11, 1782. To all appearance, the morning was very fine, but still the telescope, when first brought out, would not act well. After half an hour's exposure, it performed better.

*Confined Place.*

(42.) July 19, 1781. 13<sup>h</sup> 15'. My telescope would not act well; and, supposing the exhalations from the grass in my garden to affect vision, I carried the telescope into the street, (the observation was made at Bath,) and found it to perform to admiration. A confined place prevents telescopes from acting well.

(43.) July 19, 1781. My telescope acted very well; but a slight field-breeze springing up, and brushing through the street where my instrument was placed, it would no longer bear a magnifying power of 460.

*Haziness and Clouds.*

(44.) Sept. 22, 1783. The weather is now so hazy, that the double star  $\delta$  Cygni is but barely visible to the naked eye. This has taken off the rays of the large star, so that I now see the small one extremely well, which at other times it is so difficult to perceive, even with a magnifying power of 932. Remarkable effect of haziness and clouds.

(45.) August 13, 1781. A cloud coming on very gradually upon fixed stars, has this remarkable effect, that their apparent diameters diminish gradually to nothing.

(46.) July 7, 1780. The air was very hazy, but extremely calm. I had Arcturus in the field of view of the telescope, and, the haziness increasing, it had a very beautiful effect on the apparent diameter of this star. For, supposing the first of the points \*, to represent the magnitude when brightest, I saw it gradually decrease, and assume, with equal distinctness, the form of all the succeeding points, from No. 1 to No. 10, in the order of the numbers placed over them. The last magnitude I saw it under, could certainly not exceed two-tenths of a second; but was perhaps less than one. This leads to the discovery of one of the causes of the apparent magnitude of the fixed stars. One cause of the apparent diameters of the fixed stars.

\* These points will be inserted in one of the plates in our next number. N.

*Local*

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*Focal Length.*

Observations in which the focal length and figure of the speculum was altered by solar heat.

(47.) Nov. 14, 1801. The focal length of my 10-feet mirror increases by the heat of the sun. I have often observed this before; the difference, by several trials, amounts to 8 hundredths of an inch.

(48.) Dec. 13, 1801. The focal length of my 10-feet mirror, while I was looking at the sun, became shorter, contrary to what it used to do; but, there being a strong frost, I guess that the object metal grows colder, notwithstanding its exposure to the sun's rays.

(49.) Nov. 9, 1802. 10<sup>h</sup> 50'. The focus of my 7-feet glass mirror became 18 hundredths of an inch shorter, on being exposed for about a minute to the sun. The figure of the speculum was also distorted; the foci of the inside and outside rays differing considerably, though its curvature, by observations on the stars, has been ascertained to be strictly parabolic.

12<sup>h</sup> 0'. The same mirror, exposed one minute to the action of the sun, became 21 hundredths shorter in focal length.

The focus of a 10-feet metalline mirror, when exposed one minute to the sun's rays, became 15 hundredths of an inch longer than it was before.

(50.) January 9, 1803. When I looked with the glass 7-feet mirror, several times, a minute or two at the sun, it shortened generally .24, .26, and .30 of an inch, in focal length.

The observations which are now before us, appear to be sufficient to establish the following principle; namely,

General principle. *Uniform temperatures and moisture in the air are requisite to distinct vision.*

"That in order to see well with telescopes, it is required that the temperature of the atmosphere and mirror should be uniform, and the air fraught with moisture."

This doctrine applied to,

This being admitted, we shall find no difficulty in accounting for every one of the foregoing observations.

Sudden changes of temperature;

If an uniform temperature be necessary, a frost after mild weather, or a thaw after frost, will derange the performance of our mirrors, till either the frost or the mild weather are sufficiently settled, that the temperature of the mirror may accommodate itself to that of the air. For, till such an uniformity with the open air, in the temperature of the mirror, the tube, the eye-glasses, and I would almost add the observer, be obtained, we cannot expect to see well. See observation 15, 17, 18, 19, and 23.

But

But, when a frost, though very severe, becomes settled, the mirror will soon accommodate itself to the temperature; and we shall find our telescopes to act well. See observation 16, 20, 21, 22, 24, and 25.

This explains, with equal facility, why no telescope just or of exposure; brought out of a warm room can act properly. See observation 40 and 41.

Nor can we ever expect to make a delicate observation, <sup>observations from a confined place;</sup> with high magnifying powers, when looking through a door, window, or slit in the roof of an observatory; even a confined place, though in the open air, will be detrimental. See observation 42 and 43.

It equally shows, that windy weather in general, which must windy weather; occasion a mixture of airs of different temperatures, cannot be favourable to distinct vision. See observation 31, 32, and 33.

The same remark will apply to Auroræ Boreales, when they auroræ boreales; induce, as they often do; a considerable change in the temperature of the different regions of air. See observation 28.

But, should they not be accompanied by such a change, there seems to be no reason why they should injure vision. See observation 29 and 30.

The warm exhalations from the roof of a house in a cold <sup>the roof of an</sup> night, must disturb the uniformity of the temperature of a small <sup>house;</sup> portion of air; so that stars which are over the house, and at no considerable distance, may be affected by it. See observation 38 and 39.

Sometimes the weather appears to be fine, and yet our tele- <sup>weather appar-</sup> scopes will not act well. This may be owing to dryness occa- <sup>rently fine;</sup> sioned by an easterly wind; or to a change of temperature, arising from an agitation of the upper regions of the atmosphere. See observation 34 and 35.

Or, possibly, to both these causes combined together. See observation 36 and 37.

If moisture in the atmosphere be necessary, dry air cannot be proper for vision. See observation 26 and 27.

And therefore, on the contrary, dampness, and haziness of <sup>damp, haze;</sup> the atmosphere, must be favourable to distinct vision. See observation 1, 2, 3, 4, 6, and 8.

Fogs also, which certainly denote abundance of moisture, <sup>fogs, &c.</sup> must be very favourable to distinct vision. See observation 9, 10, 11, 12, 13, and 14.



Nay, if the observatory should be surrounded by water, we need be under no apprehension on that account. Perhaps, were we to erect a building for astronomical purposes only, we ought not to object to grounds which are occasionally flooded; the neighbourhood of a river, a lake, or other generally called damp situations. See observation 5 and 7.

It is however possible, that fogs and haziness may increase to such a degree as, at last, to take away, by their interposition, all the light which comes from celestial objects; in which case, they must of course put an end to observation; but they will nevertheless be accompanied with distinct vision to the very last. See observation 44, 45, and 46.

We have now only the four last observations to account for. They relate to the change of the focal length of mirrors in solar observations, and its attendant derangement of the foci of the different parts of the reflecting surface; and, as simplicity is one of the marks of the truth of a principle, I believe we need not have recourse to any other cause than the change of temperature produced by the action of the solar rays that occasion heat; which will be quite sufficient to explain all the phenomena. But, in order to show this in its proper light, I shall relate the following experiments.

#### *1st Experiment.*

Experiments on  
the change of  
focal length in  
mirrors by heat.

I placed a glass mirror, of 7-feet focal length, in the tube belonging to the telescope; and, having laid it open at the back, I prepared a stand, on which the iron used in my experiments on the terrestrial rays that occasion heat (see Phil. Trans. for 1800, Plate XVI. Fig. 1.) might be placed, so as to heat the mirror from behind, while I kept a certain object in the field of view of the telescope. Having measured the focal length, and also examined the figure of the mirror, which was parabolical, the heated iron was applied so as to be about  $2\frac{1}{4}$  inches from the back of the glass mirror. The consequence of this was, that a total confusion in all the foci took place, so that the letters on a printed card in view, which before had been extremely distinct, became instantly illegible. In 15 seconds, the focus of the mirror was shortened 2,3 inches; in half a minute, 3,47 inches; and, at the end of the minute, I found it no less than 4,59 inches shorter than it had been before the application of the hot iron.

On

On repeating the experiment, but placing the heated iron no more than  $\frac{1}{4}$  of an inch from the back of the mirror, its focal length, in  $1\frac{1}{2}$  minute, became 5,33 inches shorter.

Experiments on the change of focal length in mirrors by heat.

I tried also a more moderate heat; and, placing the iron at 3 inches from the back, the focus of the mirror shortened in one minute 2,83 inches.

A thermometer placed in contact with the reflecting surface of the mirror, could hardly be perceived to have risen, during the time in which the hot iron produced the alteration of the focal length.

### *2d Experiment.*

Every thing remaining as before, I suspended a small globe of heated iron in front of the mirror, at one inch and a half from its vertex; and, in two minutes, the focus was lengthened 5,3 inches. The figure of the mirror was also deranged; so that the letters on the card could not be distinguished.

I made a second trial, with the suspended iron a little more heated, and brought it as near the surface of the mirror as I judged it to be safe; since a contact would probably have cracked the mirror. In consequence of this arrangement, the focus lengthened, in one minute, 1,64 inch.

On removing the heated iron, the mirror returned, in one minute, to within ,18 inch of its former focal length; and, at the end of the second minute seemed to be nearly restored. But the disagreement of the foci of the different parts of the reflecting surface might be perceived for a long time afterwards, and caused an indistinctness of vision, which plainly indicated that, under such circumstances, the magnifying power of the telescope, 225, was more than it ought to be, in order to see well.

### *3d Experiment.*

I now changed the glass mirror for a metalline one; and, on placing the heater near the back of it, the focus of the speculum, in 30 seconds, became ,77 inch shorter. But, continuing the observation, instead of shortening still farther in the next 30 seconds, it became ,3 inch longer, so that, at the end of a minute, it was only ,47 shorter than before the approach of the hot iron.

## 4th Experiment.

Experiments on  
the change of  
focal length in  
mirrors by heat.

When the small heated globe of the 2d experiment was suspended in front of the mirror, the focus lengthened, .27 inch in one minute; nor would the lengthening increase by leaving the hot iron longer in its position. The foci in this, as well as in the 3d experiment, were so much injured that they could not be measured with any precision; and it was evident, that high magnifying powers ought not to be used with a mirror of which the temperature is undergoing a continual change.

I repeated the experiment with the iron nearly red hot; and found the focus lengthened 1.48 inch in 30 seconds. Five minutes after the removal of the iron, the regularity of the figure of the mirror was pretty well restored:

With a moderate heat, I had, in 30 seconds, a lengthening of the focus, of .57 inch; and, in about  $1\frac{1}{2}$  minute after the removal of the heated iron, distinct vision was nearly restored.

These four experiments show, that a change in the temperature of mirrors, occasioned by heat, is attended with an alteration of their focal length; and also prove, that the figure of the reflecting surface is considerably injured, during the time that such a change takes place. We are consequently authorized to believe, that the small alteration in the focus of a mirror exposed to the rays of the sun, arises from the same cause. For, since a thermometer, when the sun is shining upon it, will show that its temperature is altered, the action of the solar rays upon a mirror must be attended with a similar effect in its temperature. See observation 47, 48, 49, and 50.

The same experiments will now also explain why the observations of the sun, related in our transit of Mercury, between  $10^h 32'$  and  $11^h 28'$ , were not attended with success; for we have seen that heat occasions a derangement in the action of the reflecting surface; and it follows that, under such circumstances, high magnifying powers cannot be expected to show objects very distinctly.

If it should be remarked, that I have not explained why the focus of a glass mirror should shorten by the same rays of the sun which lengthen that of a metalline speculum, I confess that this at present does not appear; and, as it is not material to our purpose, I might pass it over in silence. We are however

pretty well assured, that the alterations of the focal length must be owing to a dilatation of the glass or metal of which mirrors are made, and must be greatest where most heat is applied. Experiments on the change of focal length in mirrors by heat. Our experiments therefore cannot agree perfectly with solar observations; for, in the glass mirror, the application of partial heat in front, must undoubtedly have been much stronger about the middle of the mirror (though the centre of it was sometimes guarded by a brass plate equal to the size of the small speculum) than at the circumference. But when, on the contrary, a mirror is exposed to the sun, every part of the surface will receive an equal portion of heat.

It may also be said, that I have pointed out a defect in telescopes used for solar observations, without assigning a cure for it. It will however be allowed, that tracing an evil to its cause must be the first step towards a remedy. Had the imperfection of the figure brought on by the heat of the solar rays been of a regular nature, an elliptical speculum might have been used to counteract the assumed hyperbolic form; or *vice versa*.

And now, as, properly speaking, the derangement of the figure of a mirror used in observing the sun, is not so much caused by the heat of its rays as by their partial application to the reflecting surface only, which produces a greater dilatation in front than at the back, there may be a possibility of counteracting this effect, by a contrary application of heat against the back, or by an interception of it on the front. But this we leave to future experiments.

### III.

*Observations on the Chemical Nature of the Humours of the Eye.*

By RICHARD CHENEVIX, Esq. F. R. S. and M. R. I. A.\*

THE functions of the eye, so far as they are physical, have been found subject to the common laws of optics. It cannot be expected that chemistry should clear up such obscure points of physiology, as all the operations of vision appear to be; but, some acquaintance with the intimate nature of the substances

\* From the Philosophical Transactions, 1803.

which

which produce the effects, cannot fail to be a useful appendage to a knowledge of the mechanical structure of the organ.

**Humours of the eye little known chemically.**  
**Aqueous humour.**

The chemical history of the humours of the eye, is not of much extent. The aqueous humour had been examined by Bertrandi; who said, that its specific gravity was 975, and therefore less than that of distilled water. Fourcroy, in his *Système des Connoissances chimiques*, tells us, that it has a saltish taste; that it evaporates without leaving a residuum; but that it contains some animal matter, with some alkaline phosphate and muriate. These contradictions only prove, that we have no accurate knowledge upon the subject.

**Vitreous humour.**

The vitreous humour is not better known. Wintringham has given its specific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to investigate its chemical nature.

**Crystalline lens.**

We are told by Chrouet, that the crystalline lens affords, by destructive distillation, fetid oil, carbonate of ammonia, and water, leaving some carbon in the retort. But, destructive distillation, although it has given us much knowledge as to animal matter in general, is too vague a method for investigating particular animal substances.

I shall now proceed to mention the experiments I have made upon all the humours. I shall first relate those which were made upon the eyes of sheep, (they being the most easily procured,) and shall afterwards speak of those of the human body, and other eyes. I think it right to observe, that all these eyes were as fresh as they could be obtained.

#### SHEEPS' EYES.

##### *Aqueous Humour.*

**Of sheeps' eyes:**  
**The aqueous humour.** Water, albumen, gelatine and muriate of soda.

The aqueous humour is a clear transparent liquid, of the specific gravity of 10090\*, at 60° of Fahrenheit. When fresh, it has very little smell, or taste.

It causes very little change in the vegetable reactive colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, some traces of which I discovered.

\* All these specific gravities are mean proportionals of several experiments. The eyes of the same species of animal, do not differ much in the specific gravity of their humours.

When

When exposed to the air, at a moderate temperature, it evaporates slowly, and becomes slightly putrid.

When made to boil, a coagulum is formed, but so small as hardly to be perceptible. Evaporated to dryness, a residuum remains, weighing not more than 8 per cent. of the original liquor.

Tannin causes a precipitate in the fresh aqueous humour, both before and after it has been boiled, and consequently shows the presence of gelatine.

Nitrate of silver causes a precipitate, which is muriate of silver. No metallic salts, except those of silver, alter the aqueous humour.

From these and other experiments it appears, that the aqueous humour is composed of water, albumen, gelatine, and a muriate, the basis of which I found to be soda.

I have omitted speaking of the action of the acids, of the alkalis, of alcohol, and of other re-agents, upon this humour. It is such as may be expected in a solution of albumen, of gelatine, and of muriate of soda.

### *CrySTALLINE Humour.*

To follow the order of their situation, the next of the humours is the crystalline.

This differs very materially from the others,

Its specific gravity is 11000.

When fresh, it is neither acid nor alkaline. It putrifies very rapidly. It is nearly all soluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid, when I had obtained the crystalline quite free from the other humours. It is composed, therefore, of a smaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

*CrySTALLINE*  
humour. Much  
larger proportion  
of albumen and  
gelatine.

### *VITREOUS Humour.*

I pressed the vitreous humour through a rag, in order to free it from its capsules; and, in that state, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its specific gravity, (which I have found to be 10000, like that of the other), or in its chemical nature,

*VITREOUS*  
humour. The  
same as the  
aqueous.

M. Fourcroy

No phosphate in these humours.

M. Fourcroy mentions a phosphate, as contained in these humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the presence of any earth, notwithstanding M. Fourcroy's assertion of that fact.

#### HUMAN EYE.

Human eye; not chemically different from other eyes.

I could not procure a sufficient quantity of these, fresh enough to multiply my experiments upon them. However, by the assistance of Mr. Carpue, Surgeon to his Majesty's Forces, I fully convinced myself, that the humours of the human eye, chemically considered, did not contain any thing different from the respective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of soda; and the crystalline humour contained only water, albumen, and gelatine. The specific gravity of the aqueous and vitreous humours, I found to be 10053; while that of the crystalline was 10790.

#### EYES OF OXEN.

So likewise the eyes of oxen.

I found the eyes of oxen to contain the same substances as the respective humours of other eyes. The specific gravity of the aqueous and vitreous humours is 10088; and that of the crystalline 10765.

Probable law; that the smaller the eye the more does the density of the crystalline differ from that of the other humours.

What is particularly worthy of notice is, that the difference which appears to exist between the specific gravity of the aqueous or vitreous humour and that of the crystalline, is much greater in the human eye than in that of sheep, and less in the eye of the ox. Hence it would appear, that the difference between the density of the aqueous and vitreous humour and that of the crystalline, is in the inverse ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments show this to be a universal law in nature, it will not be possible to deny that it is in some degree designed for the purpose of promoting distinct vision.

The crystalline is considerably more dense in approaching the centre.

In taking the specific gravity of the aqueous and vitreous humours, no particular precaution is necessary, except that they ought to be as fresh as possible. But the crystalline humour is not of an uniform density throughout; it is therefore essential, that attention be given to preserve that humour entire for this operation. I found the weight of a very fresh crystalline of an

or to be 30 grains; and its specific gravity was, as I before stated, 10765, I then pared away all the external part, in every direction, till there remained but six grains of the centre, and the specific gravity of these six grains, I found to be 11940. From this it would seem, that the density increases gradually, from the circumference to the centre.

It is not surprising that the crystalline humour should be subject to disorders, it being wholly composed of animal matter of the most perishable kind. Fourcroy says, that it is sometimes found osseous in advanced age. Albumen is coagulated by many methods; and, if we suppose that the same changes can take place in the living eye as in the dead animal matter of the chemists, it will be easy to account for the formation of the cataract; a disorder which cannot be cured but by the removal of the opaque lens. If a sufficient number of observations were made respecting the frequency of the cataract in gouty habits, some important conclusions might be drawn, as to the influence of phosphoric acid, in causing the disorder, by the common effect of acids, in coagulating albumen.

It is very subject to disorders from its coagulable nature.

#### IV.

*A Letter from Mr. IRVINE concerning the late Dr. IRVINE, of Glasgow, his Doctrine, which ascribes the Disappearance of Heat, without Increase of Temperature, to a change of Capacity in Bodies, and that of Dr. BLACK, which supposes Caloric to become latent by Chemical Combination with Bodies; with particular Remarks on the Mistakes of Dr. THOMPSON, in his Accounts of these Doctrines.*

To Mr. NICHOLSON.

SIR,

IN the article Chemistry in the Supplement to the Encyclopædia Britannica, in most respects excellently written, I could not fail to be struck with the account there given of the theory of heat, and the mode of investigating the natural zero adopted by the late Dr. Irvine. Had Dr. Thompson been indisputably accurate in his opinions, had a mathematically close argument left no door for the entrance of doubt, he would scarcely then

Account given in the Encyclopædia Brit. of the investigation of the zero of heat.



then have been justified in the terms he has used. But here we have every thing settled, and we are informed with the air of an ancient sophist, that it is examined and found insufficient. I am induced to make an observation or two on this examination, because I am informed that the mathematical air which reigns through this part of this work has actually imposed on many. For this time, however, I believe it can be shewn that it is no more than an air, and that this subject is not yet finally settled.

**Dr. Thompson's Statement of Dr. Irvine's theory of heat.** That caloric absorbed by ice on its conversion into water (without increase of temperature) only seems to become latent and is really employed in keeping up the temperature while the capacity is increased; but that this does not explain the act of fusion.

**Argument of Dr. Thompson.** The capacity of water is to that of ice as 10 : 9. —Ice during fusion absorbs what would have raised water 140°—it would therefore have raised the ice at least as much if it had remained solid—whence it is inferred that the fused ice or water ought to have been raised 9—10ths or 126°, if the difference of capacity had alone operated in the case :

Dr. Thompson says, at page 269, Suppl. Ency. Brit. " Dr. Irvine, of Glasgow, advanced a theory on this subject different from that of Dr. Black. The specific caloric of water being greater than that of ice, it requires a greater quantity of caloric to raise it to a given temperature than it does to raise ice. The caloric therefore does not become latent, it only seems to do so from the greater specific caloric of water. This theory was zealously adopted by Dr. Crawford. Dr. Black observed very justly, that it did not account for the production of fluidity at all. The specific caloric of water is indeed greater than that of ice; but how is the ice converted into water? This is an objection which the advocates for Dr. Irvine's or Dr. Crawford's theory, (as it has been improperly called) will not easily answer. Let us examine whether this theory accounts for the apparent loss of caloric. It follows from Mr. Kirwan's experiments, that the specific caloric of water is to that of ice as 10 to 9. Dr. Black proved, that as much caloric entered the ice as would have raised it had it been water, 140°. Let us suppose that it would only have raised the ice 140; in that case the melted ice ought to have been of the temperature of 158°, for 10 : 9 :: 140 : 126, but it was only 32°. Therefore 126° of caloric have disappeared, and cannot be accounted for by the change of specific caloric. Nor can the accuracy of Dr. Black's experiment be suspected; it has been repeated in every part of the world, and varied in every possible way. We cannot doubt, therefore, that caloric unites with substances, and causes them to become fluid, or that there is in fact a caloric of fluidity different from specific caloric."

Now nobody doubts Dr. Black's experiment, and it is not necessary to our argument to have any doubt on that subject. Dr. Thompson gives it as a fair statement of Dr. Irvine's theory,

ory, that the  $140^{\circ}$  entering ice during its change of forms, should be lessened in the ratio of 9 : 10, and should therefore be  $126^{\circ}$ , or in other words, if the heat entering the ice were only enough to raise the ice  $140^{\circ}$ , that the temperature of the water should be  $158^{\circ}$ . But it is easy to see that on this supposition the water would contain two portions of heat, namely, its original quantity from the natural zero to  $32^{\circ}$ , expressed in degrees according to the capacity of ice, and the superadded portion from  $32$  to  $158$ , expressed in degrees according to that of water. The ice would no doubt according to this statement, become water without absorbing any heat, and the water would be merely heated as in every other case. But Dr. Irvine, and after him Dr. Crawford, and the writers of elementary treatises for the last fifteen years, and I believe I may venture to say every one of our philosophers, except Dr. Thompson himself, have stated this doctrine of capacities to be, that every one of the thermometrical degrees expressing the whole of the heat contained in a body, are to be taken in proportion to its capacity; and therefore that if the ice suddenly changed its capacity, it would absorb not merely a rateable proportion of what heat might be presented to it, but an absolute quantity to make up for its new capacity. For the heat necessary to raise the temperature of ice each degree from natural zero, is to the heat necessary to raise water each degree from the same point as the capacity of the one body is to that of the other. Ice cannot then as it acquires its new form, shew any augmentation of temperature till the differences between the heats of each degree from the lowest point be made up. This difference amounts to the  $140^{\circ}$  found by the experiment, and cannot raise the water even the fraction of a degree in temperature, because it is barely sufficient for the demands of its new capacity. Had the  $140^{\circ}$  been applied to water, Dr. Thompson's assertion of the rise of temperature would have been just, but it can have no reference whatever to heat applied during a change of capacity.

The same arguments are repeated in page 271, and the same mistake reigns through the whole. Is it not obvious, that if you would instantaneously increase the capacity of any body, it would immediately become colder, and its temperature sink as much lower as its new capacity was higher. In the same way, if a body has its capacity suddenly increased, and

But it was not raised at all: and therefore the  $126^{\circ}$  of heat are considered as having beyond doubt combined with the body and fused it.

Reply. Dr. Thompson in his statement of the change of capacity, attends to the superadded heat only, and totally overlooks the heat before existing in the body.

Inadmissible consequence.

Dr. Irvine and all writers but Dr. Thompson have considered the whole heat in a body at a given temperature as the measure of its capacity.

Ice in fusion absorbs all the heat presented to it until its enlarged capacity is satisfied, and cannot till then have any increase of temperature.

If the capacity of a body were suddenly increased its temperature would fall; or it would continue stationary at the same if due heat were added;

and this is the  
case with ice.

hence, &c. as  
before.

The cause of  
fluidity ascribed  
to the same  
action of heat  
which enlarges  
the capacities of  
bodies.

The experi-  
ments on heat  
are mostly too  
inaccurate to  
give the same  
depression for the  
natural zero.

same time a quantity of heat added to it to make the whole heat in proportion to its new capacity, surely that body would continue exactly at the same thermometrical point. Now melting ice is that body; the  $140^{\circ}$  are demanded by new capacity,—would have been more if its capacity had been greater, and less if less. On the very principles therefore in dispute, ice on becoming water ought to receive a quantity of heat, and that quantity is not governed by the proportion of 9 to 10, but the whole heats are in that proportion, and the  $140^{\circ}$  is only their difference. For all calculations on the alteration of temperature to be produced by a given portion of heat on a body, from the knowledge of the relation of its capacity to another, and of the number of degrees that other is raised by the same quantity, continue just only while the capacities continue in the same proportion.

Surely it is possible to form a notion that heat ( $140^{\circ}$ .) may enter into water as the very cause of fusion, so as to alter its state and change its capacity; that very heat making the quantum due to the new capacity. It is the fact, that ice at  $32^{\circ}$  cannot bear the smallest addition of heat in that state, but immediately begins to be converted into water. Let a quantity much less than represented by  $140^{\circ}$ , enter a given portion of ice, say  $\frac{1}{2}$  a degree. A small portion of the ice becomes water; its capacity being increased as 9 : 10, and here the process stops; the entire mass of ice and water remaining at  $32^{\circ}$ . More heat would alter the state and enlarge the capacity of more of the ice, without raising its temperature; that is to say, would fuse it. How this is done, or in other words, what it is that happens among the particles may not be easy to explain or to imagine; but in my apprehension this hypothetical part of the discussion would be at least as obscure in the doctrine of latent or combined heat, as in that which ascribes the disappearance of heat during fusions to the enlargement of capacity.

As to the disagreement of results in the hands of different philosophers concerning the natural zero, it is to be observed, that it is one thing to determine whether bodies have different capacities for heat, and another to express these by accurate numbers. There can be no doubt of the fact that bodies have different capacities, and Dr. Irvine's theorem may enunciate itself generally, by saying, that as the capacity of the solid is  
to

to that of the fluid,\* so is the whole heat of the solid to that of the fluid. But it is a widely different thing to determine precisely the capacities of airs, and various other substances, experiments on which are subject to great inaccuracy, and must be carefully repeated many times before a philosopher should pretend to draw final, and still less elementary, conclusions. Many of the experiments by which contradictory results have been obtained were made, (as I am told, for I have not yet had an opportunity of examining myself) by the calorimeter, an instrument liable to great and deserved objections. Lavoisier, by mixing sulphuric acid and water made the natural zero 5803: my father by a similar process somewhere I think between 8 and 900. What will Dr. Thompson make of this? Surely he will not conclude the theorem false, but one experimenter wrong; most likely both inaccurate.

It is very strange indeed that Dr. Thompson should have found it difficult to understand how these 140° enter ice during fusion without raising its temperature. All he says amounts to this, that a given quantity of heat will have more effect in raising the temperature of ice than that of water. But the ice must continue ice, and the water water, and a change of capacity alters the whole reasoning. Before he can tell whether 140 should be 14 or not, he must tell me the whole heat of ice, and let me examine whether that be to itself plus, 140 as 9: 10 or not. He is exactly in the same error with regard to steam.

As to a mode of finding the capacities of ice and water, of which my father was the undoubted discoverer, as well as of the general fact that all bodies change their capacity and form together, one of Irvine's modes was this: he mixed fine river sand washed, or fine pounded glass of a given temperature with each, so as to raise or reduce each an equal number of degrees. Then the capacities were as the quantities of glass added to produce the same effect.

Dr. Irvine's method of ascertaining the capacities of ice and water was by ascertaining how much fine sand produced an equal change of temperature in each.

Dr. Thompson says, that there is no proof that the capacities of bodies are as their absolute heats. The capacity of iron, he continues, is greater than that of water or even that of azotic gas, yet it is improbable that iron contains more heat than these substances. Now where did Dr. Thompson find that the capacity of iron was greater than that of water and azotic gas? not in his own table surely. There iron by weight has capacity 0.1264, water 1.000, azotic gas, 0.7036, or as Dr Crawford

Dr. Thompson asserts that the capacities are not proved to be as the absolute heats.

Instance of iron, said to have greater capacity and contain less heat than water:—but mistakenly.

Crawford says, .7936. Even of equal Bulks that of iron is less than that of water, as I see in the same table. I can only suppose that Dr. Thompson has stumbled on the specific gravities instead of specific heats, and there he would have been right enough.

The same experiment that determines specific heats is also the test of the capacities. The absolute heats are (by inference extended thro' the whole range from zero) taken to be in the same ratio.

I will say only a few words farther on the question, whether bodies contain caloric in proportion to their specific heats or not. Now first of all when they continue of the same capacities. Suppose the capacity of a body to be to that of water as 10 to 5, *i. e.* double. The same quantity of heat that raises water two degrees, raises it one;  $2^{\circ}$  more raise the body one more, and so on as far as we can go upwards, and the reverse downwards in the scale. But suppose another body whose capacity is to that of water as 20 to 5, *i. e.* quadruple. Each  $4^{\circ}$  of the heat in water raise this new body one degree upwards, and the reverse downwards, as far as we know. Now is it not probable here that the whole heats are in proportion to the capacities thus determined, since like thermometrical portions of heat taken out of each and applied to water affect it in that ratio? The specific heats of bodies are said to be different, when the same quantity of heat raises one a different number of degrees from the other, and that regularly as far as we can examine. Therefore each degree in each contains a quantity of heat proportional to its capacity. But the whole heat is made up of degrees, therefore the whole heats are proportional if the capacities remain the same. Dr. Thompson grants this to be absolute fact to the extent of our experiments.

*If the capacities should vary while the temperature changes, the only consequence would be, that heat would be given out or absorbed till the common temperature were restored, and then the absolute heats would be proportioned to the new capacities.*

But if the capacity be supposed to vary, first let it diminish. Then the quantity of heat given out is the difference of the whole heats of the two different states of the body; and the whole heat of it in each state is proportional to its capacity, and the whole heat of its highest capacity is equal to that of its lowest plus the heat given out. Therefore the change of capacity has made no alteration on the whole heat of the body computed from a higher point, but will turn out the same as if no change had taken place.

If the capacity be supposed to increase, a similar reasoning would shew that the heat still may be computed in the same way. Such are a few arguments on the other side of the question.

tion from Dr. Thompson, by no means all that might be brought, nor dare I venture to hope so stated as to be beyond the reach of censure.

Many more curious points rife before the imagination on so interesting a subject as heat. As I hope, however, soon to be able to lay before the public some of my father's writings, I may on that occasion have an opportunity of expressing myself at greater length than I can intrude in your journal.

I am,  
SIR,

Your obedient humble servant,  
WILLIAM IRVINE.

Bedford-Street, Covent-Garden.

## V.

*An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables; and on their Operation in Tanning. By HUMPHRY DAVY, Esq. Professor of Chemistry in the Royal Institution.*

(Concluded from Page 256. Vol. V.)

### IV. EXPERIMENTS AND OBSERVATIONS ON THE ASTRINGENT INFUSIONS OF BARKS, AND OTHER VEGETABLE PRODUCTIONS.

THE barks that I examined were furnished me by my friend Samuel Purkis, Esq. of Brentford; they had been collected in the proper season, and preserved with care.

Infusions of  
barks in water  
by gentle heat.

In making the infusions, I employed the barks in coarse powder; and, to expedite the solution, a heat of from 100 to 120° Fahrenheit was applied.

The strongest infusions of the barks of the oak, of the Leicester willow, and of the Spanish chestnut, were nearly of the same specific gravity, 1.05. Their tastes were alike, and strongly astringent; they all reddened litmus-paper; the infusion of the Spanish chestnut bark producing the highest tint; and that of the Leicester willow bark the feeblest tint.

Of oak, willow,  
and Spanish  
chestnut,

Two hundred grains of each of the infusions were submitted to evaporation; and, in this process, the infusion of the oak

were chemical-  
ly examined.

Bark

Chemical examination of various barks.

bark furnished 17 grains of solid matter; that of the Leicester willow about  $16\frac{1}{2}$  grains; and that of the Spanish chefnut nearly an equal quantity.

The tannin given by these solid matters was, in that from the oak bark infusion, 14 grains; in that from the willow bark infusion  $14\frac{1}{2}$  grains; and in that from the Spanish chefnut bark infusion 13 grains.

The residual substances of the infusions of the Spanish chefnut bark, and of the oak bark, slightly reddened litmus-paper, and precipitated the solutions of tin of a fawn colour, and those of iron black. The residual matter of the infusion of the willow bark, did not perceptibly change the colour of litmus; but it precipitated the salts of iron of an olive colour, and rendered turbid the solution of nitrate of alumine.

The solid matters produced by the evaporation of the infusions, gave, by incineration, only a very small quantity of ashes, which could not have been more than  $\frac{1}{15}$  of their original weights. These ashes chiefly consisted of calcareous earth and alkali; and the quantity was greatest from the infusion of chefnut bark.

The infusions were acted on by the acids, and the pure alkalis, in a manner very similar to the infusion of galls. With the solutions of carbonated alkalis, they gave dense fawn-coloured precipitates. They were copiously precipitated by the solutions of lime, of strontia, and of barytes; and, by lime-water in excess, the infusions of oak and of chefnut bark seemed to be deprived of the whole of the vegetable matter they held in solution.

By being boiled for some time with alumine, lime, and magnesia, they became almost colourless, and lost their power of acting upon gelatine and the salts of iron. After being heated with carbonate of lime and carbonate of magnesia, they were found deeper coloured than before; and, though they had lost their power of acting on gelatine, they still gave dense olive-coloured precipitates with the salts of iron.

In all these cases, the earths gained tints of brown, more or less intense.

When the compound of the astringent principles of the infusion of oak bark with lime, procured by means of lime-water, was acted on by sulphuric acid, a solution was obtained, which

pre-

precipitated gelatine, and contained a portion of the vegetable principles, and a certain quantity of sulphate of lime; a solid fawn-coloured matter was likewise formed, which appeared to be sulphate of lime, united to a little tannin and extractive matter.\*

The solutions were copiously precipitated by solution of albumen. Copious precipitations by albumen.

The precipitates they gave with gelatine were similar in their appearance; their colour, at first, was a light tinge of brown, but they became very dark by exposure to the air. Their composition was very nearly similar; and, judging from the experiments on the quantity of gelatine employed in forming them, the compound of tannin and gelatine from the strongest infusion of oak bark, seems to consist, in the 100 parts, of 59 parts of gelatine and 41 of tannin; that from the infusion of Leicester willow bark, of 57 parts of gelatine and 43 of tannin; and that from the infusion of Spanish chefnut bark, of 61 parts of gelatine and 39 of tannin.

Two pieces of calf-skin, which weighed when dry 120 grains each, were tanned; one in the strongest infusion of Leicester willow bark, and the other in the strongest infusion of oak bark. Experiments of tanning skin with the infusions of barks. The process was completed, in both instances, in less than a fortnight; when the weight of the leather formed by the tannin of the Leicester willow bark was found equal to 161 grains; and that of the leather formed by the infusion of oak bark was equal to 164 grains.

When pieces of skin were suffered to remain in small quantities of the infusions of the oak bark, and of the Leicester willow bark, till they were exhausted of their tanning principle, it was found, that though the residual liquors gave olive-coloured precipitates with the solutions of sulphate of iron, yet they were scarcely rendered turbid by solutions of muriate of tin; and there is every reason to suppose, that a portion of their extractive matter had been taken up with the tannin by the skin. Spent ounce of infusion.

\* M. Merat Guillot proposes a method of procuring pure tannin, (*Annales de Chimie*, Tome XLI. p. 325.) which consists in precipitating a solution of tan by lime-water, and decomposing it by nitric or muriatic acid. The solution of the solid matter obtained in this way in alcohol, he considers as a solution of pure tannin; but, from the experiments above-mentioned, it appears, that it must contain, besides tannin, some of the extractive matter of the bark; and it may likewise contain saline matter.



I attempted, in different modes, to obtain uncombined gallic acid from the solid matter produced by the evaporation of the barks, but without success. When portions of this solid matter were exposed to the degree of heat that is required for the production of gallic acid from Aleppo galls, no crystals were formed; and the fluid that came over gave only a brown colour to the solution of salts of iron, and was found to contain much acetous acid and empyreumatic oil.

When pure water was made to act, in successive portions, upon oak bark in coarse powder, till all its soluble parts were taken up, the quantities of liquor last obtained, though they did not act much upon solution of gelatine, or perceptibly red-dened litmus-paper, produced a dense black with the solution of sulphate of iron: by evaporation, they furnished a brown matter, of which a part was rendered insoluble in water by the action of the atmosphere; and the part soluble in water was not in any degree taken up by sulphuric ether; so that, if it contained gallic acid, it was in a state of intimate union with extractive matter.

Slow tanning appears to add less to the weight of the leather than quick, and more of the mucilage is taken up.

Two pieces of calf-skin, which weighed when dry 94 grains each, were slowly tanned; one by being exposed to a weak infusion of the Leicester willow bark, and the other by being acted upon by a weak infusion of oak bark. The process was completed in about three months; and it was found, that one piece of skin had gained in weight 14 grains, and the other piece about  $16\frac{1}{2}$  grains. This increase is proportionally much less than that which took place in the experiment on the process of quick tanning. The colour of the pieces of leather was deeper than that of the pieces which had been quickly tanned; and, to judge from the properties of the residual liquors, more of the extractive matters of the barks had been combined with them.

The experiments of Mr. Biggin\* have shown, that similar barks, when taken from trees at different seasons, differ as to the quantities of tannin they contain: and I have observed, that the proportions of the astringent principles in barks, vary considerably according as their age and size are different; besides, these proportions are often influenced by accidental circumstances, so that it is extremely difficult to ascertain their distinct relations to each other.

## EXPERIMENTS ON ASTRINGENT VEGETABLES.

In every astringent bark, the interior white bark (that is, the part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part: but the epidermis seldom furnishes either tannin or extractive matter.

The interior coat of bark contains most tannin; the middle most extractive; and the epidermis little of either.

The white cortical layers are comparatively most abundant in young trees; and hence their barks contain, in the same weight, a larger proportion of tannin than the barks of old trees. In barks of the same kind, but of different ages, which have been cut at the same season, the similar parts contain always very nearly the same quantities of astringent principles; and the interior layers afford about equal portions of tannin.

Young trees afford more tannin.

An ounce of the white cortical layers of old oak bark, furnished, by lixiviation and subsequent evaporation, 108 grains of solid matter; and, of this, 72 grains were tannin. An equal quantity of the white cortical layers of young oak produced 111 grains of solid matter, of which 77 were precipitated by gelatine.

Relative quantities.

An ounce of the interior part of the bark of the Spanish chestnut, gave 89 grains of solid matter, containing 63 grains of tannin.

The same quantity of the same part of the bark of the Leicester willow, produced 117 grains, of which 79 were tannin.

An ounce of the coloured or external cortical layers from the oak, produced 43 grains of solid matter, of which 19 were tannin.

From the Spanish chestnut, 41 grains, of which 14 were tannin.

And, from the Leicester willow, 34 grains, of which 16 were tannin.

In attempting to ascertain the relative quantities of tannin in the different entire barks, I selected those specimens which appeared similar with regard to the proportions of the external and internal layers, and which were about the average thickness of the barks commonly used in tanning, namely, half an inch.

Of these barks, the oak produced, in the quantity of an ounce, 61 grains of matter dissolved by water, of which 29 grains were tannin.

The Spanish chestnut 53 grains, of which 21 were tannin.

And the Leicester willow 71 grains, of which 33 were tannin.

The proportions of these quantities, in respect to the tanning principle, are not very different from those estimated in Mr. Biggin's table.\*

Properties of the residual portion of infused barks.

The residual substances obtained in the different experiments, differed considerably in their properties; but certain portions of them were, in all instances, rendered insoluble during the process of evaporation. The residuum of the chefnut bark, as in the instance of the strongest infusion, possessed slightly acid properties; but more than  $\frac{1}{4}$  of its weight consisted of extractive matter. All the residuums in solution, as in the other cases, were precipitated by muriate of tin; and, after this precipitation, the clear fluids acted much more feebly than before on the salts of iron; so that there is great reason for believing, that the power of astringent infusions to precipitate the salts of iron black, or dark coloured, depends partly upon the agency of the extractive matters they contain, as well as upon that of the tanning principle and gallic acid.

Elm and willow bark.

In pursuing the experiments upon the different astringent infusions, I examined the infusions of the bark of the elm and of the common willow. These infusions were acted on by reagents, in a manner exactly similar to the infusions of the other barks: they were precipitated by the acids, by solutions of the alkaline earths, and of the carbonated alkalis; and they formed, with the caustic alkalis, fluids not precipitable by gelatine.

An ounce of the bark of the elm, furnished 13 grains of tannin.

The same quantity of the bark of the common willow, gave 11 grains.

The residual matter of the bark of the elm, contained a considerable portion of mucilage; and that of the bark of the willow, a small quantity of bitter principle.

Infusions of sumachs from Sicily and Malaga.

The strongest infusions of the sumachs from Sicily and Malaga, agree with the infusions of barks, in most of their properties; but they differ from all the other astringent infusions that have been mentioned, in one respect; they give dense precipitates with the caustic alkalis. Mr. Proust has shown, that sumach contains abundance of sulphate of lime; and it is probably to this substance that the peculiar effect is owing.

From an ounce of Sicilian sumach, I obtained 165 grains of matter soluble in water, and, of this matter, 78 grains were tannin.

\* Philosophical Transactions for 1799, p. 263.

An ounce of Malaga sumach, produced 156 grains of soluble matter, of which 79 appeared to be tannin.

The infusion of Myrobalans \* from the East Indies, differed <sup>Of Myrobalans,</sup> from the other astringent infusions chiefly by this circumstance, that it effervesced with the carbonated alkalis; and it gave with them a dense precipitate, that was almost immediately redissolved. After the tannin had been precipitated from it by gelatine, it strongly reddened litmus-paper, and gave a bright black with the solutions of iron. I expected to be able to procure gallic acid, by distillation from the Myrobalans; but in this I was mistaken; they furnished only a pale yellow fluid, which gave merely a slight olive tinge to solution of sulphate of iron.

Skin was speedily tanned in the infusion of the Myrobalans; and the appearance of the leather was similar to the appearance of that from galls.

The strongest infusions of the teas are very similar, in their agencies upon chemical tests, to the infusions of catechu.

An ounce of Souchong tea, produced 48 grains of tannin.

The same quantity of green tea, gave 41 grains.

Dr. Maton has observed, that very little tannin is found in cinchona, or in the other barks supposed to be possessed of febrifuge properties. My experiments tend to confirm the observation. None of the infusions of the strongly bitter vegetable substances that I have examined, give any precipitate to gelatine. And the infusions of quassia, of gentian, of hops, and of chamomile, are scarcely affected by muriate of tin; so that they likewise contain very little extractive matter.

In all substances possessed of the astringent taste, there is great reason to suspect the presence of tannin; it even exists in substances which contain sugar and vegetable acids. I have found it in abundance in the juice of sloes; and my friend Mr. Poole, of Stowey, has detected it in port wine.

#### V. GENERAL OBSERVATIONS.

Mr. Proust has supposed, in his paper upon tannin and its <sup>Probability that</sup> Species,† that there exist different species of the tanning principle, <sup>there are different species of</sup> possessed of different properties, and different powers <sup>tanning matter.</sup>

\* The Myrobalans used in these experiments are the fruit of the *Terminalia Chebula*. RETZ. *Obs. Botan.* Fasc. V. p. 31.

† *Annales de Chimie*, Tome XLI. p. 332.

of acting upon re-agents, but all precipitable by gelatine. This opinion is sufficiently conformable to the facts generally known concerning the nature of the substances which are produced in organised matter; but it cannot be considered as proved, till the tannin in different vegetables has been examined in its pure or insulated state. In all the vegetable infusions which have been subjected to experiment, it exists in a state of union with other principles; and its properties must necessarily be modified by the peculiar circumstances of its combination.

The specific agencies of tannin in all infusions are the same.

From the experiments that have been detailed it appears, that the *specific* agencies of tannin in all the different astringent infusions are the same. In every instance, it is capable of entering into union with the acids, alkalis, and earths; and of forming insoluble compounds with gelatine, and with skin. The infusions of the barks affect the greater number of re-agents in a manner similar to the infusion of galls; and, that this last fluid is rendered green by the carbonated alkalis, evidently depends upon the large proportion of gallic acid it contains. The infusion of sumach owes its characteristic property, of being precipitated by the caustic alkalis, to the presence of sulphate of lime; and, that the solutions of catechu do not copiously precipitate the carbonated alkalis, appears to depend upon their containing tannin in a peculiar state of union with extractive matter, and uncombined with gallic acid or earthy salts.

Its affinities and habitudes.

In making some experiments upon the affinities of the tanning principle, I found that all the earths were capable of attracting it from the alkalis: and, so great is their tendency to combine with it, that, by means of them, the compound of tannin and gelatine may be decomposed without much difficulty; for, after pure magnesia had been boiled for a few hours with this substance diffused through water, it became of a red-brown colour, and the fluid obtained by filtration produced a distinct precipitate with solution of galls. The acids have less affinity for tannin than for gelatine; and, in cases where compounds of the acids and tannin are acted on by solution of gelatine, an equilibrium of affinity is established, in consequence of which, by far the greatest quantity of tannin is carried down in the insoluble combination. The different neutral salts have, comparatively, feeble powers of attraction for the tanning principle; but, that the precipitation they occasion in astringent solutions,

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is not simply owing to the circumstance of their uniting to a portion of the water which held the vegetable substances in solution, is evident from many facts, besides those which have been already stated. The solutions of alum, and of some other salts which are less soluble in water than tannin, produce, in many astringent infusions, precipitates as copious as the more soluble saline matters; and sulphate of lime, and other earthy neutral compounds, which are, comparatively speaking, insoluble in water, speedily deprive them of their tanning principle.

From the different facts that have been stated, it is evident that tannin may exist in a state of combination in different substances, in which its presence cannot be made evident by means of solution of gelatine; and, in this case, to detect its existence, it is necessary to have recourse to the action of the diluted acids.

It is not always exhibitable by gelatine,

In considering the relations of the different facts that have been detailed, to the processes of tanning and of leather-making, it will appear sufficiently evident, that when skin is tanned in astringent infusions that contain, as well as tannin, extractive matters, portions of these matters enter, with the tannin, into chemical combination with the skin. In no case is there any reason to believe that gallic acid is absorbed in this process; and M: Seguin's ingenious theory of the agency of this substance, in producing the deoxygenation of skin, seems supported by no proofs. Even in the formation of glue from skin, there is no evidence which ought to induce us to suppose that it loses a portion of oxygen; and the effect appears to be owing merely to the separation of the gelatine, from the small quantity of albumen with which it was combined in the organized form, by the solvent powers of water.

Skins in tanning absorb extractive matter;

The different qualities of leather made with the same kind of skin, seem to depend very much upon the different quantities of extractive matter it contains. The leather obtained by means of infusion of galls, is generally found harder, and more liable to crack, than the leather obtained from the infusions of barks; and, in all cases, it contains a much larger proportion of tannin, and a smaller proportion of extractive matter.

and the leather has its quality affected by it;

When skin is very slowly tanned in weak solutions of the barks, or of catechu, it combines with a considerable proportion of extractive matter; and, in these cases, though the increase

Soft durable leather by slow tanning.

of

of weight of the skin is comparatively small, yet it is rendered perfectly insoluble in water ; and is found soft, and at the same time strong.

The saturated astringent infusions of barks contain much less extractive matter, in proportion to their tannin, than the weak infusions ; and, when skin is quickly tanned in them, common experience shows that it produces leather less durable than the leather slowly formed.

The common opinion in favour of what is called feeding of the leather probably just.

Besides, in the case of quick tanning by means of infusions of barks, a quantity of vegetable extractive matter is lost to the manufacturer, which might have been made to enter into the composition of his leather. These observations show, that there is some foundation for the vulgar opinion of workmen, concerning what is technically called the *feeding* of leather in the slow method of tanning ; and, though the processes of the art may in some cases be protracted for an unnecessary length of time, yet, in general, they appear to have arrived, in consequence of repeated practical experiments, at a degree of perfection which cannot be very far extended by means of any elucidations of theory that have as yet been made known.

On the first view it appears singular that, in those cases of tanning where extractive matter forms a certain portion of the leather, the increase of weight is less than when the skin is combined with pure tannin ; but the fact is easily accounted for, when we consider that the attraction of skin for tannin must be probably weakened by its union with extractive matter ; and, whether we suppose that the tannin and extractive matter enter together into combination with the matter of skin, or unite with separate portions of it, still, in either case, the primary attraction of tannin for skin must be, to a certain extent, diminished.

Vegetables are of value for tanning not merely by the matter that glue can precipitate.

In examining astringent vegetables in relation to their powers of tanning skin, it is necessary to take into account, not only the quantity they contain of the *substance* precipitable by gelatine, but likewise the quantity, and the nature, of the extractive matter ; and, in cases of comparison, it is essential to employ infusions of the same degree of concentration.

Catechu is the most powerful tanning material.

It is evident, from the experiments detailed in the III<sup>d</sup> section, that of all the astringent substances which have been as yet examined, catechu is that which contains the largest proportion of tannin ; and, in supposing, according to the common estimation, that from four to five pounds of common oak bark

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are required to produce one pound of leather, it appears, from the various synthetical experiments, that about half a pound of catechu would answer the same purpose\*.

Also, allowing for the difference in the composition of the different kinds of leather, it appears, from the general detail of facts, that one pound of catechu, for the common uses of the tanner, would be nearly equal in value to  $2\frac{1}{2}$  pounds of galls, to  $7\frac{1}{2}$  pounds of the bark of the Leicester willow, to 11 pounds of the bark of the Spanish chestnut, to 18 pounds of the bark of the elm, to 21 pounds of the bark of the common willow, and to 3 pounds of sumach.

Various menstruums have been proposed for the purpose of expediting and improving the process of tanning, and, amongst them, lime-water and the solutions of pearl-ash: but, as these two substances form compounds with tannin which are not decomposable by gelatine, it follows that their effects must be highly pernicious; and there is very little reason to suppose, that any bodies will be found which, at the same time that they increase the solubility of tannin in water, will not likewise diminish its attraction for skin.

## VI.

*An easy Method of raising Water for the Purposes of Refrigeration in Distilleries, Steam Condensers, &c. By SIR A. N. EDEL-CRANTZ. Communicated by the Inventor.*

THE method exhibited in the sketch, Fig. II. Plate IV. being capable of saving near thirty feet of the height to which water may be required to be pumped, for the uses mentioned in the title, appeared too simple and ingenious, when the learned inventor did me the honour to mention it in conversation, for me not to require permission to communicate it to my readers.

\* This estimation agrees very well with the experiments lately made by Mr. Purkis, upon the tanning powers of Bombay catechu in the processes of manufacture, and which he has permitted me to mention. Mr. Purkis found, by the results of different accurate experiments, that one pound of catechu was equivalent to seven or eight of oak bark.



The principle of the syphon applied to produce a current of water through a worm tub.

If the worm tube were open at the top as usual, it is evident that all the water employed for cooling, would require to be raised by some mechanic force as high as the surface; suppose twenty feet. But as this water is not wanted for use at that elevation, but is only required to give out its heat, and then fall down again; it is clear that this fall may be applied to raise a considerable portion of what is to follow. Various means might be devised for such an application; the simplest and most effectual, no doubt, is that to which Sir A. N. has given the preference, namely to convert the whole apparatus into a syphon.

Description of the apparatus.

Suppose the worm tub to be closed at top; the cold water conveyed into it at the bottom from the vessel A, and carried off heated at top by the pipe B into the overflowing vessel C. Let us suppose the level in A to be two feet higher than that in C, and a current will be kept up through the whole fluid as long as may be desired.

Whether the escape of gas would prevent or impede the effect.

It must occur to the experienced engineer that gas or air will escape from the water, especially when heated and defended from the pressure of the atmosphere. But this may be obviated by attending to a few necessary circumstances. First, the elevation need not be very great, and less gas will escape; secondly, the temperature may be kept down by a large current through pipes of considerable diameter; and thirdly, it is practicable by various contrivances, that an interior float shall give notice when the gas has lowered the surface of the water beneath it to a certain point, and this may either warn an attendant to pump it out, or it may discharge an apparatus to produce the same effect without the immediate exertion of labour each individual time.

Concerning these and other secondary points, I shall not, however, enlarge; having enough to regret from the necessary imperfection of this description, taken from the conversation of the inventor, instead of being given in his own words.

W. N.

## VII.

*Description of a new Padlock of Security with Combinations.* By  
CITIZEN REGNIER.\*

**T**HE intention of this padlock is to secure portmanteaus, cloak bags, and other packages in the most complete manner, and to serve occasionally as defences to the key-holes of the doors of apartments. Description of a padlock of combination.

The padlock is composed of four circular pieces of brass, on which are engraven the twenty-four letters of the alphabet. The four pieces are moveable on their axes by turning them with the finger in order to produce the combination by which it is opened.

The combination of the manufacturer is the word ROME : when this word is brought into a correct line with the two marks on the edges of the two steel plates FF, which form the external part of the padlock, those two plates can be separated a little from each other, and the clasp of the lock can be opened by the hinge.

The same process is used to fasten it, with this difference, that the two external plates are pressed together so as to confine the bow or clasp of the lock in its cell at G ; after which, the combination is to be shifted so that the characters shall no longer form the same word in the before-mentioned line.

*The Method by which the Possessor may dispose the Padlock to act by a new Combination, which cannot be known to any other Person :*

1. A screw is taken out, which passes through the centre of the plates FF.

2. The combination which it is intended to set aside, namely, that which opens the lock at present must be duly arranged.

3. The marked circular pieces or rings must be taken off from four plates of brass which constitute the central part, and together form the centre-piece of the mechanism.

4. Lastly, The rings must be replaced on the centre pieces, each according to the letter the possessor may have chosen.

For example : If you would adopt the word LOCK for the combination, the letter L of the first ring must be placed over

\* Translated from a paper circulated by the constructor.

**Description of a padlock of combination.** or upon a small steel tooth, which is attached to the first interior cylinder; the letter O of the second ring on the tooth of the second cylinder; the letter C of the third ring on the tooth of the third cylinder; and lastly, the letter K of the fourth tooth on the fourth cylinder.

By this means the word *lock* is set up and becomes the combination of the lock, and the word *Rome* no longer produces the disposition of parts required for the disengagement.

After this operation the screw must be replaced in the centre of the plate; this screw does not contribute to the strength of the mechanism; but is used merely to allow the exact space necessary for opening the padlock, and to prevent the separation of the rings from the central parts in the common use of the lock.

*Method of using this Lock as a Defence to the Key-hole of a Door.*

A ring staple A, having a wood screw, is fixed to the door above the key-hole or escutcheon of the lock.

A second ring C is fixed perpendicularly beneath the other.

A cylindrical tube of iron D, in the form of a bolt, is placed vertically in the ring of these screw staples. At the lower end of the tube is an aperture, through which the padlock is inserted, so that the tube or bolt cannot be raised or taken out.

By this contrivance the key-hole of the lock is completely defended, and the introduction of a pick-lock or false key is rendered morally impossible. For the mechanism presents 331,776 combinations, \* forming 331,775 different obstacles, to prevent the removal of this defence by any person unacquainted with the secret of the proprietor.

If it be apprehended that the word of the combination may be forgotten, it will be easy to write and disguise it in many different ways, without any risk of discovery: for example,

The letter L, or eleventh letter of the alphabet	£.
	will be written 11.
The letter O, or fourteenth letter	- - - - 14.
The letter C, or third letter	- - - - 3.
The letter K, or tenth letter	- - - - 10.
Total (expressed)	£. 38.

\* Number equal to the 4th power of 24.

This little calculation will appear to any other person to be a common account, but it is to the proprietor a memorandum by which he will perfectly recollect that the first letter of his combination is the eleventh of the alphabet, that the second is the fourteenth, and so of the rest.

Description of a  
padlock of com-  
bination.

REMARKS.

This padlock appears at first sight similar to that contrived by Cardan; but we know that his was not capable of having its combinations changed; whence it results that the manufacturer, the retailer, and every other person who may have seen it opened, can themselves open it with the same facility as the proprietor himself. The notches which produce the opening may be also discovered by the feel; our lock has false notches cut in the centre-piece of the mechanism which prevent the discovery of the real one.

Lastly, The clasp of this padlock is made of hardened and tempered steel, to prevent its being easily cut by an ordinary file. It is annealed so far only as to prevent its breaking.

ANNOTATION. W. N.

The remaining part of this paper contains the address of the inventor and vendor, C. Regnier, *ci devant Jacobins, Rue Dominique, F. St. Germain à Paris*, and also a certificate of honourable mention, &c. from the *Athénée des Arts*.

The lock of Cardan consists of the four visible circular parts carrying the alphabet. These as well as the central parts of the present lock are perforated half way through their centers by an hole, and quite through by a smaller hole, in the side of which last there is a notch extended to the circumference of the larger hole. All the four pieces are placed upon a central pin, which has side projections answering respectively to the notches, but occupying the space of the larger hole while the lock is closed. From this construction it is evident that the lock cannot be opened unless every one of the notches be placed opposite its projection; that this position or placing is settled by the maker, and not variable; and that the lock is liable to be opened, though not easily, by the tentative process described at p. 204 of our last volume.

Cit. Regnier has perfected the lock of Cardan by making the system of the alphabet moveable with regard to the internal

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Description of a  
padlock of com-  
bination.

nal notch at the pleasure of the possessor, and also by making grooves or small notches on the face of each central piece, which answer the purpose of the teeth recommended at the page last quoted, by preventing the rings from being turned round while any pull is made against the closure.

I find some obscurity in his description of the manner of connecting the central piece and the external engraved part. From the operation, I apprehend, 1. that each ring has a number of notches at its inner surface, that answer to the letters on its outer face; 2. that each central round piece fits the cavity of its ring, and is prevented from turning by a tooth which it lodges in one of the notches; 3. that when all the four teeth are ranged in a line between F and F, the lock will open; and therefore, 4. when any particular letter is placed over the tooth, that letter becomes the effective letter for its own ring.

## VIII.

*Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By WILLIAM HYDE WOLLASTON, M. D. F. R. S.\**

Former paper  
of the author,  
and of Monge,  
upon horizontal  
refraction.

IN a Paper which I some time since presented to this Society, (printed in the Phil. Transf. for 1800,) I endeavoured to ascertain the causes, and to explain the various cases, of horizontal refraction, which I had either observed myself, or had seen described by others.

At the time of writing that essay, I had not met with the *Mémoires sur l'Égypte*, published but a short time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of seeing, in their march through the deserts of Egypt.

In the perusal of this memoir, I could not fail to derive instruction from the information it contained; but, as the facts related by him accord entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has proposed, in preference to my own.

\* From the Philosophical Transactions for 1803.

The definite reflecting surface which he supposes to take place between two strata of air of different density, is by no means consistent with that continued ascent of rarefied air which he himself admits; and the explanation founded on this hypothesis will not apply to other cases, which may all be satisfactorily accounted for, upon the supposition of a gradual change of density, and successive curvature of the rays of light by refraction.

Objection to the theory of Monge.

I have since learned that the same subject has also been ably treated by Mr. Woltman, in Gilbert's *Annalen der Physik*; but I have to regret that his dissertation, as well as that of Gruber, in the same Annals, were written in a language that was unknown to me, and that I could not avail myself of the assistance that I might otherwise have received from their researches.

The subject well treated by Woltman.

When I formerly engaged in this inquiry, being impressed with the advantage to be derived from it to nautical astronomy, on account of the variations in the dip of the apparent horizon, from which all observations of altitude at sea must necessarily be taken, I suggested the expediency of a series of observations, to be made by a person attentive to those changes of temperature or moisture of the atmosphere, on which he might find the depression of his horizon principally to depend. I had at that time no expectation that I could myself pursue this subject farther to any useful purpose, having little prospect of residing for a sufficient length of time in view of the sea, and seeing no other method by which the same end might be accomplished. I have, however, since that time, found means to satisfy myself, by observations over the surface of the Thames, that although the quantity of refraction varies in general with any change of the thermometer or hygrometer, yet the law of these variations is not altogether so simple as I had hoped it might be found.

Observations over the surface of the Thames.

I shall, on the present occasion, first relate the facts on which this opinion is founded, and which are in themselves sufficiently remarkable, on account of the unexpected quantity of refraction observable over a short extent of water; I shall, in the next place, shew that the exact determination of the concurrent changes of the atmosphere are of less value, and their irregularities of less consequence, than I had conceived, as there is a very easy method whereby the quantity of dip at sea may be at any time correctly measured; and therefore the end which I sought by indirect means, may be at once directly attained.

Narrative.

The

Apparent distortion of the parts of vessels seen over the surface of the Thames.

The first instance that occurred to me, of observable refraction over the surface of the Thames, was wholly accidental. I was sitting in a boat near Chelsea, in such a position that my eye was elevated about half a yard from the surface of the water, and had a view over its surface, that probably somewhat exceeded a mile in length, when I remarked that the oars of several barges at a distance, that were then coming up with the tide, appeared bent in various degrees, according to their distance from me. The most distant appeared nearly in the form represented, Plate I. Fig. 1. *dd* being my visible horizon by apparent curvature of the water; *ab* the oar itself in its inclined position; and *bc* an inverted image of the portion *bc*. By a little attention to other boats, and to buildings on shore, I could discern that the appearance of all distant objects seen near the surface of the water was affected in a similar manner, but that scarcely any of them afforded images so perfectly distinct as the oblique line of an oar dipped in the water.

These effects are different from what might be caused by reflection.

A person present at the time (as well as some others to whom I have since related the circumstance) was inclined to attribute the appearance to reflection from the surface of the water; but, by a moderate share of attention, a very evident difference may be discovered between the inversion occasioned by reflection, and that which is caused by atmospherical refraction. In cases of reflection, the angles between the object and image are sharp, the line of contact between them straight and well defined, but the lower part of the image indefinite and confused, by means of any slight undulation of the water. But, when the images are caused by refraction, the confines of the object and its inverted image are rounded and indistinct, and the lower edge of the image is terminated by a straight line at the surface of the water. In addition to these marks of difference, there is another circumstance which, if attended to, must at once remove all doubt; for, by bringing the line of sight near to the surface of the water, boats and other small objects are found to be completely hidden by an apparent horizon, which, in so short a distance, cannot be owing to any real curvature of the water, and can arise solely from the bending of the rays by refraction.

They appear referrible to an higher temperature in the

When I reflected upon the causes which were probably instrumental in the production of these phenomena, they appeared referrible

referrible to difference of temperature alone. After a succession of weather so hot that the thermometer, during one month preceding, had been 12 times above  $80^{\circ}$ , and on an average of the month at  $68^{\circ}$ , the evening of that day (August 22, 1800) was unusually cold, the thermometer being  $55^{\circ}$ . The water might be supposed to retain the temperature it had acquired during a few weeks preceding, and, by warming the stratum of air immediately contiguous to it, might cause a diminution of its refractive density, sufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with instruments of any kind, I had it not in my power to estimate the quantity of refraction, or temperatures; and can only say that, to my hand, the water felt in an uncommon degree warmer than the air.

water, which being communicated to the lowest stratum of the air, alters its refractive power.

Being thus furnished with an unexpected field for observation, I from that time took such opportunities as similar changes of the weather afforded me, of examining and measuring the quantities of refraction that might be discovered by the same means over another part of the river, that I found most suited to my convenience.

Particular account of the observations.

The situation from which the greater part of my observations were made, was at the S. E. corner of Somerset house. The view from this spot extends under Blackfriars bridge, towards London bridge, upwards of a mile in length, and in the opposite direction through Westminster bridge, which is three quarters of a mile distant.

Such distances are however by no means necessary; and indeed the air over the river, in cold weather, is generally, or at least very frequently, not sufficiently clear for seeing distinctly to so great distances. For, since the winds which are most likely to effect a sufficient change of temperature, on account of their coldness, are usually from the E. or N. E. the principal smoke of the town is then brought in that direction, and hovers, like a dense fog over the course of the river. This circumstance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpose, and obliged me often to make use of shorter distances than I should otherwise have chosen, by bringing the line of sight as near as I could to the surface of the water.

For this purpose, I had a plane reflector fitted to the object-end of a small pocket telescope, at an angle of  $45^{\circ}$ , so that,



when the telescope was held vertically, it gave a horizontal view at any level that was found most eligible. When the water has been calm, I have observed that the greatest refraction was visible within an inch or two of its surface, and I have then seen a refraction of six or seven minutes in the space of 300 or 400 yards: at other times, I have found it greatest at the height of a foot or two; but, in this case, a far more extensive view becomes necessary.

The first measures that I took were on the 23d of September, 1800. The water was  $2\frac{1}{2}^{\circ}$  warmer than the air, and I found a refraction of about 4'.

Oct. 17. The difference of temperature was  $3^{\circ}$ , and the refraction 3'.

Oct. 22. The water was  $11\frac{1}{2}^{\circ}$  warmer than the air, yet the quantity of refraction did not exceed 3'.

The smallness of the quantity of refraction upon this occasion, I attributed to the dryness of the atmosphere, conjecturing that a rapid evaporation might in great measure counteract that warmth which the water would otherwise have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added also the degrees of cold produced by keeping the bulb of it moistened for a sufficient time to render it stationary. In confirmation of my conjecture respecting the dryness of Oct. 22, I have also, in the following Table, which comprises the whole of my observations, inserted a column from the Register kept at the apartments of the Royal Society, containing the heights of the hygrometer, on those mornings when my observations were made.

TABLE.

At 8, A. M.	Air.	Water.	Difference.	Refraction.	Cold by evaporation.	Hygrometer.	Table of observations.
1800. Sept. 23	57	60 $\frac{1}{2}$ °	3 $\frac{1}{2}$ °	4'	— —	72°	
Oct. 17	46 $\frac{1}{2}$	49 $\frac{1}{2}$	3	3	— —	72	
22	38	49 $\frac{1}{2}$	11 $\frac{1}{2}$	3	— —	67	
Nov. 1	41	45 $\frac{1}{2}$	4 $\frac{1}{2}$	8	$\frac{1}{2}$ °	76	
4	43 $\frac{1}{4}$	46 $\frac{1}{4}$	3	3 —	1 $\frac{1}{4}$	72	
5	37	45	8	8 +	1	69	
12	44 $\frac{1}{2}$	48 $\frac{1}{2}$	4	1 +	3 $\frac{1}{2}$	73	
13	40	44 $\frac{1}{2}$	4 $\frac{1}{2}$	5	$\frac{1}{2}$	76	
1801. June 13	50	63	13	9 +	5	65	
22	55	61	6	6 +	6	65	
23	55	62	7	6	4 $\frac{1}{2}$	65	
24	55	61	6	5	3	67	
Sept. 8	60	64	4	7	2	78	
9	64	64 $\frac{1}{4}$	$\frac{1}{4}$	5	3	74	
10	58	64	6	7	2	70	
12 o'clock, 10	63	64	1	2			

From a review of the preceding Table it will be found, upon the whole, that when the water is warmer than the air, some increase of depression of the horizon may be expected; but that its quantity will be greatly influenced, and in general diminished, by dryness of the atmosphere.

It appears, however, that no observable regularity is deducible from the measures above given; but that the quantity, on some occasions, is far different from what the states of the thermometer and hygrometer would indicate. On the 9th of September, for instance, the difference of temperature is only  $\frac{1}{4}$ °, and the evaporation, to counteract this slight excess of warmth, produced as much as 3° of cold; nevertheless, the refraction visible was full 5'. In this observation I think that I could not be mistaken, as the water was at the time perfectly calm, the air uncommonly clear, and I had leisure to pay particular attention to so unforeseen an occurrence.

This one instance appears conformable to the opinion entertained by Mr. Huddart, and by M. Monge, that, under some circumstances, the solution of water in the atmosphere causes a decrease in its refractive power; but, on no other occasion have I been induced to draw a similar inference.

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *ec*, Fig. 1. Pl. 3.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

It is not likely that so great a variation of the dip happens out at sea.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and the increase of dip caused by this variation of horizontal refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

Mr. Huddart's remedy.

The remedy employed by Mr. Huddart,\* of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above  $180^\circ$  as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and, when it exceeds  $30^\circ$ , the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

\* Phil. Trans. for 1797, p. 40.

On account of the difficulty attending some of the adjustments for the back observation, he rejects that method for taking angles in general, with much reason; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occasion to hurry the most inexperienced or cautious observer.

By the back observation, the whole vertical angle between any two opposite points of the horizon may be measured at once, either before or after taking an altitude. Half the excess of this angle above  $180^\circ$ , should of course be the dip required.

Objections to the back observation of angles, but the method recommended for measuring the arc between the opposite horizons.

But, if it be doubtful whether the instrument is duly adjusted, a second observation becomes necessary. The instrument must be reversed, and, if the apparent deficiency of the opposite angle from  $180^\circ$  be not equal to the excess before obtained, the index error may then be corrected accordingly; and, since the want of adjustment, either of the glasses at right angles to the plane of the instrument, or of the line of sight parallel to it, will affect both the larger and smaller angle very nearly in an equal degree, the  $\frac{1}{2}$  part of their difference will be extremely near the truth, and the errors arising from want of those adjustments may with safety be neglected.

Error of the glasses found by reversing the whole instrument.

This method of correcting the index error for the back observation at sea, was many years since recommended by Mr. Ludlam; \* yet I do not find that it has been noticed by subsequent writers on that subject, or suggested by any one for determining the dip; but I can discover no reason for which it could be rejected as fallacious, and I should hope that in practice it would be found convenient, since in theory it appears to be effectual.

as first suggested by Ludlam.

The most obvious objection to this, as well as to Mr. Huddart's method, is the possibility that the refraction may be in some measure different in opposite points of the horizon at the same time. When land is at no great distance, such an inequality may be found to occur; but, upon the surface of the ocean in general, any partial variations of temperature can rarely be supposed to exist; and it is probable, that under any circumstances, the difference will not bear any considerable

Whether refraction may be different on different bearings at the same time.

\* Directions for the use of Hadley's quadrant, 1771, § 82, p. 56.

proportion to the whole refraction; nor can it be thought a sufficient reason for rejecting one correction proposed, that there may yet remain other small errors, to which all methods are equally liable, but which it is not the object of the present dissertation to rectify.

IX.

*An Account of Two Halos, with Parhelia. By Sir H. C. ENGLEFIELD, Bart. F. R. S\*.*

Uncommon  
halos round the  
sun.

ON the 20th of November, 1802, at two o'clock in the afternoon, going out of doors, at Richmond in Surry, I perceived the sun, accompanied by uncommon halos and parhelia. The weather was showery, and the sky had that peculiar turbid appearance, which is the certain forerunner of heavy and long continued rain. The sun shone with a faint and watery light, was very ill defined, and appeared rather elongated in a vertical direction. A very dense cloud occupied all that quarter of the horizon, and rose up pretty near to the sun. Very heavy clouds covered the eastern part of the heavens, extending quite to the north, and were proceeding gradually towards the south west. The wind was about east.

The altitude of the sun was  $14^{\circ}$ . The circle nearest the sun was distant from him nearly  $24^{\circ}$ , and was about a degree in breadth. It was of a pale yellowish light, but had no tendency to prismatic colours.

On the left hand, it extended below a line drawn through the sun parallel to the horizon. To the right, it terminated in dense clouds considerably above that line.

The exterior circle was  $48^{\circ}$  from the sun, and it might be  $1\frac{1}{2}^{\circ}$  in breadth, as it was evidently broader than the inner circle. It terminated on either hand at nearly the same height as the interior one. It was tinged throughout with the prismatic colours, though only red, green, and blue, were distinctly visible. The red was nearest the sun. The blue very faint. The brightness of this circle was about that of the secondary rainbow, to a bright common bow.

In a line parallel to the horizon, passing through the sun, there was, in the left hand branch of the inner circle; a very faint parhelion; but in the upper point of the same circle, there was a very bright and remarkable one. Its light was so vivid, that it could scarcely be steadily viewed; and, indeed, it was rather brighter than the real sun. It was of a whiter light than the rest of the circle in which it was, and had a pearly appearance, as partaking a little of prismatic tints. It was large, perhaps in its brightest part near two degrees broad, very ill defined every where, but most diffused in the part furthest from the sun. From each side of the bright light proceeded a bright ray, which had a double curvature very distinct, being first convex towards the sun, and then concave. The lower edge of these rays (or that nearest the sun), was tolerably well defined, the upper edge melted away into the sky, with a sort of streakiness. They grew both narrower and fainter towards their termination, and they reached pretty near to the other circle.

Uncommon  
halos round the  
sun.

The whole form of this parhelion and its rays, bore so striking a similitude to the body and extended wings of a long winged bird, such as an eagle, hovering directly over the sun, that superstition would really have had little to add to the image.

There was no trace of any other circle or arch in the heavens, nor of any anthelion.

It is probable, that it had been still more beautiful before I saw it, as during the time I observed it, its brightness was continually diminishing; some traces, however, were visible for nearly half an hour.

The measures which I have given must be considered as very rough. I had no instrument at hand, but a six inch pocket sector. I held the joint of this close to my eye as I could, and opened it, till the points of the legs coincided with the sun and with the circles that I wished to measure. I am, however, inclined to think, that the measures I have given are true within a degree.

The accompanying sketch, (Plate III.) which is drawn on a scale of  $20^{\circ}$  to an inch, from a rough draught which I made at the moment, will give a more distinct idea of the whole appearance than can be conveyed by words.

*A Theory*

*A Theory of Halos and Parhelia.* By THOMAS YOUNG,  
M. D. F. R. S.

**Theory of halos and parhelia.** The explanation of the primary and secondary rainbow begun by De Dominis, and completed by Descartes and Newton, derives an entire and satisfactory confirmation, from the perfect coincidence of the observed angular magnitudes, with the result of calculations of the effect of spherical drops. We know that drops of water, either accurately, or very nearly spherical, exist in great abundance in every cloud, and in every shower of rain; and whatever their dimensions may be, they must necessarily conspire in the same general effect, of producing the same rainbow, whenever a spectator is placed in a proper situation for observing it; consequently such rainbows are of very frequent occurrence.

**Variable halos produced by equal drops.** I have attempted to show, that for producing the phenomena of variable halos, often observable in hot climates, it is only necessary that a considerable part of the spherules of a cloud or mist, be either accurately, or very nearly, of equal magnitude, a condition, of which the possibility is easily admitted from analogy, and the probability is favoured by the apparent uniformity of the different parts of such mists as we can examine.

**The constant halo of  $23^{\circ}$  not explained.** But no satisfactory reason has hitherto been assigned for the production of the halo, which in these climates is the most common of all; that is, the constant halo of  $23^{\circ}$  or  $24^{\circ}$ . The hypothesis by which Huygens attempted to explain the production of halos and parhelia, are both arbitrary and improbable. He imagined the existence of particles of hail, some globular, others cylindrical, with an opaque part in the middle of each, bearing a certain ratio to the whole; and he supposed the position of the cylinders to be sometimes vertical, and sometimes inclined to the horizon in a given angle.

**The hypothesis of Huygens improbable.** It has already been objected, that no such particles have ever been observed to accompany halos; and it is, besides, highly improbable, that such an opaque part should bear the same proportion in all the hailstones, and that the cylinders should have terminations so peculiar as is supposed; and the most incredible circumstance of all is, that all these proportions should be constantly such, as always to produce a halo at the distance of  $23^{\circ}$  or  $24^{\circ}$  from the sun or moon.

It

It appears, that a much simpler and more natural explanation of these phenomena may be deduced from the regular crystallization of snow in the atmosphere.

It is well known, that the crystals of ice and snow, tend always to form angles of  $60^\circ$ ; now a prism of water or ice, of  $60^\circ$ , produces a deviation of  $23^\circ 37'$ , for rays forming equal angles with its surfaces, and the angle of deviation varies at first very slowly, as the inclination changes, the variation amounting to less than  $3^\circ$ , while the inclination changes  $30^\circ$ .

The equilateral prismatic crystals of ice produce a deviation of  $23^\circ 37'$  which varies at first very slowly.

Now if such prisms were placed at all possible angles of inclination, differing equally from each other, one half of them would be so situated, as to be incapable of transmitting any light regularly by two successive refractions directed the same way; and of the remaining two fourths, the one would refract all the light within these three degrees, and the other would disperse the light in a space of between  $20^\circ$  and  $30^\circ$  beyond them.

The casual arrangement of these will give the halo.

In the same manner, we may imagine an immense number of prismatic particles of snow to be disposed in all possible directions, and a considerable proportion of them to be so situated, that the plane of their transverse section may pass within certain limits of the sun and the spectator. Then half of these only will appear illuminated, and the greater part of the light will be transmitted by such as are situated at an angular distance of  $23^\circ 37'$ , or within  $3^\circ$  of it: the limit being strongly marked internally, but the light being externally more gradually lost. And this is precisely the appearance of the most common halo. When there is a sufficient quantity of the prismatic particles, a considerable part of the light must fall, after one refraction, on a second particle; so that the effect will be doubled: and, in this case, the angle of refraction will become sufficient to present a faint appearance of colour, the red being internal, as the least refrangible light, and the external part having a tinge of blue.

Refraction through two prisms gives the greater halo of  $47^\circ$ .

These concentric halos of  $23\frac{1}{2}^\circ$  and  $47^\circ$ , are therefore sufficiently explicable, by particles of snow, situated promiscuously in all possible directions. If the prisms be so short as to form triangular plates, these plates, in falling through the air, will tend to assume a vertical direction, and a much greater number of them will be in this situation than in any other. The reflection from their flat surfaces will consequently produce a horizontal circle of equal height with the sun; and their refraction

Very short prisms or plates will fall edge-wise; and the reflection of their ends will give the horizontal circle, and their reflection a parheliion with wings.

will



will exhibit a bright parheliion immediately over the sun, with an appearance of wings, or horns, diverging upwards from the parheliion.

Exp. with the  
prism.

For all such particles as are directed nearly towards the spectator, will conspire in transmitting the light much more copiously than it can arrive from any other part of the circle; but such as are turned more obliquely, will produce a greater deviation in the light, and at the same time a deflection from the original vertical plane. This may be easily understood, by looking at a long line through a prism, held parallel to it: the line appears, instead of a right line, to become a curve, the deviation being greater in those rays that pass obliquely with respect to the axis of the prism; which are also deflected from the plane in which they were passing.

The line viewed through the prism has no point of contrary flexure, but if its ordinates were referred to a centre, as in the case of the halos, it would assume a form similar to that which Sir Henry Englefield has described.

Tho' snow flakes  
are complicated,  
their elements  
may be simple in  
the upper  
regions, &c.

The form of the flakes of snow as they usually fall, is indeed more complicated than we have been supposing, but their elements in the upper regions of the air are probably more simple. The coincidence in the magnitude of the observed and calculated angles is so striking, as to be nearly decisive with respect to halos, and it is not difficult to imagine that many circumstances may exist, which may cause the axis of the greater number of the prisms to assume a position nearly horizontal, which is all that is required for the explanation of the parhelia with their curved appendages. Perhaps also, the effect may sometimes be facilitated by the partial melting of the snow into conoidal drops: for it may be shown, by the light of a candle transmitted through a wine glass full of water, that such a form is accommodated to the production of an inverted arch of light, like that which is frequently observed to accompany a parheliion.

## X.

*A Description of DR. YOUNG'S Apparatus for illustrating the Doctrine of Preponderance\*. Plate I. Fig. II.*

ALTHOUGH there can be no doubt of the truth of the mathematical conclusions, which have been deduced from the well known laws of motion, respecting the most advantageous employment of force in machines, yet they have, in general, been too little considered in practical works, and scarcely ever enforced by experimental illustration. The apparatus contrived for this purpose, has been mentioned in the account of the lectures on mechanics; its advantage is derived from the simplicity of its operation, and the facility of observing at once the several motions, which begin at the same time, and may easily be compared, as long as they continue. The ratio of the portions of the middle pulley, which is that of 5 to 2, is near enough to the maximum ( $\sqrt{2} \times 1$ ): 1; and the other ratios 3:2 and 4:1 are taken sufficiently different from this to show that the velocity of each is inferior to that of the middle pulley. The pulleys are all perforated in the axis, and move freely on a strong polished wire, supported by two short arms, projecting a little from two upright pieces about three feet in length, in order that the descending weights may proceed without interruption beyond the edge of the table.

Apparatus of pulleys for showing the most advantageous employment of force.

*An Account of an Experiment on the Velocity of Water flowing through a Vertical Pipe. By the same Author.*

IT has been asserted by some writers on hydraulics, and Venturi describes a particular experiment in support of the assertion, that the discharge of water running out of the bottom of a cistern, through a descending pipe, is nearly the same as if the cistern were continued through the whole height, from the surface of the water to the orifice of the pipe, and the water were then discharged from the bottom of the cistern by a short pipe in any direction. The apparent difficulty of finding a cause adequate to the effect, on the one hand, and the

Whether the discharge of water through a vertical pipe from the bottom of a cistern be the same as from an hole in a cistern of the total depth.

\* In the lectures of the Royal Institution from whose Journal No. 11. the present and next articles are taken.

authority

authority of Venturi on the other, made it desirable that the experiment should be repeated; and an apparatus Fig. III. Plate I. was constructed, in the house of the Royal Institution, for performing it in a simple and satisfactory manner. The cistern employed was a cube of nine inches: close to the bottom a cylindrical tube was inserted, in a horizontal direction, nine inches in length, and half an inch in diameter; another tube, of exactly the same dimensions, was provided with a flat funnel at its upper end, and its lower end was fitted to slide in a collar placed in one of the upper angles of the cistern, so that it was supported in a vertical position. Water was poured into the funnel, as fast as it could be transmitted through the tube, and, as the surface of the fluid rose in the cistern, the vertical tube was drawn up, so that its lower orifice was barely immersed in the water. It was expected, that if the velocity of the water in the vertical tube were equal to the velocity corresponding to half its length, the water in the cistern would stand at the height of four inches and a half, or one half of that length, and that the pressure of this head of water would generate, in the water flowing through the horizontal tube, nearly the same velocity as the column of water would acquire in its descent through the vertical tube: the friction and resistance being in both cases the same.

### Experiment

shewed the contrary.

But the result was far different, and it fully confirmed the truth of the received theory: for the water rose in the cistern to the height of eight inches, which was very nearly the length of the tube. It is true that the water had already some velocity when it entered the funnel; but most of this must have been lost by reflection from its sides and bottom; and the quantity of air bubbles, that were unavoidably carried down with the water, must have fully compensated the little that remained.

The entire adhering column in the tube renders the atmosphere active upon the surface of the fluid, and produces the same effect as an head of that height.

It appears therefore, that we are to consider this effect in a light somewhat different from that in which it was placed in the lectures on hydraulics. The water acquires all its velocity, in consequence of the pressure of the atmosphere acting jointly with its cohesion, in a very small space at the entrance of the tube: consequently, during the whole time of its descent it acquires no new motion, and the whole force of its gravitation must therefore be at liberty to act in any other way; hence the whole column produces the same degree of pressure as if it were at rest, and causes the atmosphere to press on the water

above

above it in proportion to its whole height, in the same manner as if the pressure were derived in any other way from an equal column of water; and the case is reduced to a perfect analogy with the pressure of a head of water of this height, since the air acts upon the particles entering the tube in the same manner as the water does in more common cases. Had the result of the experiment been different, it would have been an exception to the general principle of the preservation of living force, or the equality of the potential ascent to the actual descent; for, the water moving with the velocity due to half the height only, would have been capable of ascending but to half the height.

## XI.

*Account of a simple Eudiometric Apparatus constructed and used by Dr. T. C. HOPE, F. R. S. Edin. &c. &c.*

SINCE the discovery of the uncertainty with which the application of nitrous gas to atmospheric air, and other mixtures containing oxygen is attended, it has been found desirable to present solid or liquid substances for the absorption of that principle. This on first consideration may seem at least as easy to be done as to mix two gases; but it is by no means so, because the liquids in particular possess a degree of chemical activity which renders it inconvenient to immerse the hands in them, or to expose their surface to the open air. Dr. Hope, whom I had lately the pleasure of seeing in town, mentioned an apparatus he uses in his lectures and experiments, which is at once simple and effectual, and I am happy in his permission to describe it in this place.

A Fig. 3. Plate IV. represents a bottle which may be  $1\frac{1}{2}$  inch in diameter, and  $2\frac{1}{2}$  inches in length, having a neck and stopper at D, and another neck as usual at C, into which the neck of the bottle or body B, is fitted by grinding. This last was made of the same diameter as the bottle, but seven or eight inches long. B contains the gas, and A the liquid; for example, solution of hydrosulphuret. When B is thus connected with A, the compound vessel may be inverted and agitated;

The uncertainty of eudiometrical experiments with nitrous gas renders the use of other substances necessary.

Apparatus by which fluids can be applied to air without inconvenience.

tated; and the liquid will flow into B, where it will absorb the oxygen, and form a partial vacuum. If this circumstance be found, or apprehended, to prevent the complete or rapid absorption, the vessel A may be plunged beneath the surface of common water, and the stopper D slowly opened. The pressure of the atmosphere will then force in a quantity of water, which will dilute the hydrofulphuret, but not sufficiently to prevent the completion of the process. The vessel B must be graduated to show the dimensions of the residue, or otherwise this residual gas may be transferred into a vessel expressly graduated for measuring gases.

The apparatus simply and highly convenient.

By this simple and elegant apparatus we see that the liquid is economized, and the facility, neatness, and precision of experiment insured. The size here mentioned is very well adapted to the purposes of public demonstration; but it is almost needless to remark, that it may be made considerably smaller without depriving it of its utility and excellence.

P. S. While reading this proof, I have received a line from Dr. Hope, by which I am very sorry to find that the sketch I have given is not accurate; but as it is now late in the month, and the figure is engraved, I shall be careful to give another engraving with the observations he may favour me with. W. N.

## SCIENTIFIC NEWS.

*Combustion of Metals in non-respirable Gases, by means of Galvanism.\**

Metals burned in nitrogen, &c. by galvanism.

PROFESSOR Tromsdorff has noticed that metals are combustible by means of the galvanic spark in hydrogen, ammonia, nitrogen, nitrous and carbonic acid gases.

### *Reduction of the Oxide of Titanium.*

Reduction of titanium.

Professor Lampadius has succeeded in reducing to the metallic state by means of charcoal only, the oxide of titanium, obtained by decomposing the gallate of titanium by potash or soda. The metallic titanium is of a dark copper colour; it

\* Tromsdorff's History of Galvanism and its chemical agency, p. 122.

has much metallic brilliancy, is brittle, and possesses in small scales a considerable degree of elasticity. It tarnishes on exposure to air, and becomes easily oxidized by heat. It then acquires a blueish aspect. It detonates with nitrate of potash, and is highly infusible. All the dense acids act upon it with considerable energy. *Scherer's Journ.* IX. p. 49. p. 72.

*On the Precipitability of the Oxide of Bismuth.*

Mr. Buckholtz has found that the solution of bismuth prepared in the cold is alone decomposable, by a copious addition of water, but that no such effect takes place in the solution prepared by means of heat. He has also noticed that a solution of this metal prepared in the cold, deposits its oxide in a crystalline form merely by warming the solution gradually.

*Scherer IX. p. 73.*

*New Method of preparing phosphate of Soda.*

Mr. Funcke, apothecary at Linz, in Germany, has discovered a new method of preparing phosphate of soda, in a more economical, expeditious and easy manner, than any of the processes hitherto made use of by manufacturers or chemists. His process consists, in saturating the excess of lime contained in calcined bones with dilute sulphuric acid, and then dissolving the remaining phosphate of lime in nitric acid. To this solution, he adds a like quantity of sulphate of soda, and then recovers the nitric acid by distillation. The phosphate of soda is then separated from the sulphate of lime, by the affusion of water, and crystallization in the usual manner. *Scherer IX. 59.*

**REMARK BY THE TRANSLATOR. (A.)**

This process seems to be much preferable to that now in use, viz. to decompose the bones of animals burned to whiteness, by sulphuric acid, and then presenting soda to the disengaged phosphoric acid. For the phosphate of lime cannot be completely decomposed by the affusion of sulphuric acid; on account of this acid forming instantly a portion of sulphate of lime: the liberated phosphoric acid then produces with the remaining undecomposed portion of phosphate of lime, a sub-phosphate of lime, which cannot be decomposed by sulphuric acid, and which together with the sulphate of lime already produced, forms an

unmanageable and bulky mass. And again if to this mass, washed out with water as well as possible, carbonate of soda be now presented, a partial decomposition will only be effected, for it is the excess of the phosphoric acid of this salt only, which in that case forms the article sought. The remaining portion of phosphoric acid remain united to the lime, in the form of phosphate of lime. The above process is therefore evidently better.

*Sulphate of Soda prepared from Sulphate of Lime.*

**Sulphate of Soda from gypsum.** This method consists in making into a paste with a sufficient quantity of water, eight parts of burned gypsum, or sulphate of lime, five of clay, and five of common salt. This mixture is burned in a kiln or other convenient oven, and then ground to powder, diffused in a sufficient quantity of water, which after being strained and evaporated, is suffered to crystallize.

Scherer IX. 61.

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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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OCTOBER, 1803.

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ARTICLE I.

*Analysis of the Egyptian Heliotropium; a Mineral lately imported from that Country. By FREDERICK ACCUM, Practical Chemist, and Teacher of Chemistry. Communicated by the Author.*

A PARTICULARLY beautiful species of silicious stone Beautiful  
has lately been imported from Egypt, which was stated in the ous stone of a  
letter of the person who sent it, to possess the peculiar property green colour  
of reflecting the rays of the sun red, when immersed in water; from Egypt.  
and when taken out of this fluid to exhibit the figures of the  
sun and of the moon, when viewed in a particular direction.  
But as none of the purchasers of this mineral could make out  
these singular properties, the price fixed on the article was  
considerably diminished, and the stone sold at a cheap rate.  
The beautiful green colour which it possesses, and the capabi-  
lity of receiving a high polish, together with the facility of  
cutting it, has nevertheless rendered it a convenient article for  
being worked into different objects of fancy and ornament.

The colour of this stone is a fine apple-green. It is very External appear-  
hard, and cannot be scratched, but with the point of a good ance and prop-  
pen-knife. Its fracture is even and free from all asperities. ties.  
It breaks with very sharp edges, and its texture is very com-  
pact. It is semi-transparent when in pieces not exceeding half an



inch in thickness. It strikes fire with steel, and breaks into acute angular irregular pieces. Its specific gravity is 2,708. The stone is sold by the venders by the name of *Egyptian Heliotropium*. Its nature will be more clearly deduced from the following examination :

### *Analysis.*

#### *Analysis.*

Ignition rendered it of a chocolate brown, with white and yellow veins; the polish uninjured; loss 1.21st part.

Diffusion of the powdered stone in four parts potash, fusion, solution of the mass in water, rather turbid.

Saturated with muriatic acid; gelatinous mass. Dilution with water.

Evaporation nearly to dryness, addition of dilute muriatic acid; precipitate, *filix*.

Residual fluid concentrated and saturated with carbonate of potash.

An entire polished piece of the stone, weighing 250 grains, was exposed to a white heat, in a wind-furnace, for two hours, and then suffered to cool. The original green colour of the mineral, was changed during this process to a chocolate brown, with snow white and lemon yellow veins. The polish of the stone was not injured, but its weight was diminished 12 grains.

Five hundred grains of the ignited stone were finely pulverized, and mixed with 2000 grains of potash, dissolved in a like quantity of water. This mixture was evaporated to dryness in a crucible of platina, and exposed to heat, gradually increased to whiteness, for one hour. During this operation the mixture fused quietly, and exhibited when cold, a homogeneous opaque mass, of a reddish colour. This mass was covered with water, and after having been exposed to a gentle heat, a solution of it was effected in that fluid.

This alkaline solution was a little turbid, and could not be rendered transparent by repeated filtration. I saturated it with muriatic acid, a white precipitate fell down, the whole fluid acquired a reddish hue, and assumed a gelatinous consistence. The latter could but difficultly be diminished, by a copious admixture of water.

In order to collect the precipitate which was diffused through the fluid, the whole was evaporated nearly to dryness, and then transferred into muriatic acid, diluted with six times its quantity of water, and afterwards filtered. The precipitate, after having been washed, dried, and ignited, weighed 365 grains. It was pure *filix*.

The fluid from which these earths had been separated, together with the water expended for washing it, I concentrated by evaporation to about  $\frac{1}{3}$  of its original bulk, and then saturated it with a heated solution of carbonate of potash in excess. The white precipitate which was deposited, I transferred into a flask containing muriatic acid; a brisk effervescence ensued, but no perfect solution could be effected, by either this, or any other

other acid employed. It was therefore filtered, and the insoluble part collected. This insoluble residue was of a fine red colour, and harsh to the touch. It weighed  $2\frac{1}{4}$  grains. But after having been boiled in muriatic acid, it lost  $\frac{1}{4}$  grain, and acquired a perfect white colour. The acid made use of for that purpose proved to have stripped it of iron to that amount. It was therefore a portion of flint which had eluded the first process employed for separating this earth.

Insoluble (red) portion boiled with muriatic acid gave a little iron, and left flint.

The muriatic solution from which this siliceous earth had been separated, I mingled with a solution of carbonate of potash, till no further turbidness ensued. The obtained precipitate was transferred after repeated ablutions in water, into a boiling solution of potash, and digested in that fluid for one hour. The mixture was then diluted with water, and the insoluble part separated by the filter.

The last muriatic solution treated as before.

The alkaline solution was saturated with muriatic acid, and decomposed by carbonate of ammonia in excess; the precipitate obtained by this means, after having acquired a considerable consistence, was digested in acetic acid, and examined for magnesia, but no vestige of such earth could be detected. It was therefore dried and ignited, its weight amounted to  $20\frac{1}{4}$  grains.

The alkaline solution was decomposed by carbonate of ammonia, and digested in acetic acid, and the solution contained no magnesia, but only alumina.

The insoluble residue from which this earth had been separated, was again dissolved in muriatic acid, and into this solution I dropped liquid ammonia till the odour of the latter considerably predominated. A brown flocculent precipitate fell down, which was collected, washed in liquid ammonia, and boiled for a few minutes in a solution of potash. It was then transferred into a small retort, and nitric acid affused upon it, which was again distilled off from it repeatedly. The retort was lastly heated to a dull redness. The precipitate now weighed 29 grains.

The residue not taken up by the acetic acid was again dissolved in mur. acid, and precip. by excess of ammonia.

The fluid from which this precipitate had been obtained, together with the solution of potash made use of for boiling it, was then mixed with muriatic acid, and afterwards decomposed by the addition of carbonate of potash. The product obtained, effervesced with muriatic acid, and yielded sulphate of lime on assaying a small quantity of it by sulphuric acid. It was therefore dried, and exposed to a white heat, after which its weight was  $56\frac{1}{2}$  grains.

This precip. after perfect separation of all acid, and complete oxidation by nitric acid, was weighed as oxide of iron.

The fluid from which the iron had been separated was mixed with mur. acid and decomposed by carb. potash. Lime fell down.

The residuary fluid was further examined in the usual manner, but no other substance could be found, but what had been introduced during the different processes made use of in this examination. The analysis of the mineral being therefore completed, from which it appears that 500 grains of the Egyptian heliotropium contain,

Component parts of Egyptian heliotropium.	Silex - - - - -	365 grains
	Alumine - - - - -	20 $\frac{1}{4}$
	Oxide of Iron - - - - -	29 $\frac{1}{4}$
	Lime - - - - -	56 $\frac{1}{2}$
	Water - - - - -	24.
		495
		Lofs 5
		<hr/> 500

Old Compton-Street, Soho,  
Sept. 8th, 1803.

P. S. In my paper on the compound of phosphorus and sulphur in your last Journal (August) I observe a typographical error, p. 5. l. 22. which materially affects the sense, and which I will thank you to correct, namely, for  $\frac{1}{3}$ , read  $\frac{1}{37}$  part.

F. A.

## II.

*Method of closing wide mouthed Vessels intended to be kept from communicating with the Air. In a Letter from ANTHONY CARLISLE, Esq.*

To Mr. NICHOLSON

DEAR SIR,

**Closure of wide mouthed vessels containing anatomical preparations.** IT is frequently desirable to close the openings of wide-mouthed vessels intended to contain substances which would be injured by free exposure to the atmosphere, or to evaporation. The present observations, however, originate in attempts made to improve the art of preserving anatomical preparations.—The most usual liquids employed for what are termed “wet preparations,” are weak ardent spirits, and distilled oil of turpentine,

pentine, to which may be added an aqueous solution of hydragryrus muriatus, or mercury corrosive sublimate in the proportion of twenty grains of the latter to a pound of pure water.

The methods ordinarily adopted for closing glass vessels used *Usual methods.* for these purposes, have been ground glass stoppers, well soaked bladders with a middle plate of thick sheet lead, to keep the top level, and plates of glass luted with glaziers putty.

The objections to these methods are found by experience to *Objections to* be sufficient to induce the trial of others. The ground glass *each severally.* stoppers are seldom air-tight, but when they are, it happens that by the accumulating of particles of dust in the fitting, the stopper becomes in a few years immoveable. Where oil of turpentine is employed, the stopper becomes fixed by hardened turpentine. The stopping with bladders and sheet lead is liable to such repeated changes of pressure within and without, by the alterations in the expansion of the contained liquids from variations of temperature; that the cohesion of the bladders are eventually destroyed; add to this, that such preparations cannot be taken out of the vessels for examination, or the liquor renewed, without the trouble of a new stopping. The plate of glass with putty is seldom air tight, but when it is so, the stopping is liable to the same objection which was stated lastly against the bladders.

The method I have now adopted, is to have a glass jar with *New method by* a groove half an inch deep round the outside of the top or *a jar, the cover* mouth, and a glass lid, like that used by confectioners in their *of which fits* show glasses, the lid fitting loosely into the groove is rendered *into a groove* air tight by hog's lard, a substance never quite fluid at the *with hog's lard.* highest temperature of this climate, and always soft enough in the cold season to admit of removing the lid or top.

The first glass of this kind was made to my order by Mr. Parker in Fleet-street, to contain twenty ounce measures of water, and the cost was five shillings. A similar adjustment for the lids of earthen jars, to contain pickles, preserves, &c. seems both eligible and easy in practice. See Plate V. Fig. 2.

I am,

DEAR SIR,

Your obliged Friend,

A. CARLISLE.

*Soho-Square.*

*Extract*

## III.

*Extract of a Letter from Toulon to General le Vavasseur, Inspector of the Materials of the Guns of the French Navy, on the Changes which Cast Iron undergoes by remaining long in the Sea.\**

Cast iron lying thirty years at the bottom of the sea was oxidized, Not uniformly, but in veins.

A cannon sunk in a ship burned at the evacuation of Toulon was oxidized only in the middle.

AN observation I have never heard explained is, that cast iron, which has lain a long time at the bottom of the sea, is not equally oxidized throughout. I formerly saw a cannon weighed up, after it had been sunk thirty years, which was so much oxidized in veins, that I could run a knife into some places, while the metal close by was impenetrable; and on carrying the knife beyond this hard vein, it entered as before. A gun has just been weighed up here (at Toulon,) belonging to one of the ships burnt when the English evacuated the city. The middle is so uniformly oxidized, that a large piece may be cut off with a hatchet. Toward the breech, and toward the mouth, the metal appears to have lost nothing of its hardness. Can this difference be ascribed to the contact of the substances in which the gun was buried underneath the water? Its position at the bottom of the sea not being known, we can form no conjectures on this point. For my part I had imagined, from the hard veins of the cannon mentioned above, that its metal had intermixed with it substances on which salt water could not act. The gun lately taken out of the sea appeared more homogenous, but I cannot frame any satisfactory explanation of the fact.†

\* *Annales de Chimie*, V. 133.

† It is probable, that the last cannon, as it belonged to a ship that was burned, had part of it heated to such a point, when it fell into the sea, as would occasion it to be oxidized in a higher degree than the others. It appears to me, more difficult to explain the different veins exhibited by the former cannon. *Note of General le Vavasseur.*

IV.

*On the Antiquity of the Invention of Gun-powder, and its first Application to Military Purposes. By Mr. WIEGLEB.\**

THE period of the invention of Gun-powder, and its first application for the purpose of artillery, has not yet been accurately ascertained. Though there are many accounts which have been given concerning the invention of this destructive compound, yet none of them state their authorities. The uncertainty concerning the invention of gun-powder seems merely to be owing to the want of proper documents. The most summary accounts the author of this paper could find, are contained in two Essays, the one written by Gram, the other by Tremler. The first account contains the history of the invention of gun-powder in Europe, and its first application in Denmark; the latter comprehends both the invention of powder, and the use of guns among the Europeans.

Historical facts to determine the first invention of gunpowder, and its use in war.

According to the opinion of Gram, gun-powder was already known in Europe about the year 1340. Tremler, on the contrary, endeavours to prove, that no author of credit has positively shewn that it was known before the year 1354. These authors, therefore, differ only 14 years, respecting the period of the invention. Being the other day, says Mr. Wiegleb, in search of some documents deposited in the archives of this town (Langensalze), I happened to meet with the annual account of the expences of the town for the year 1378. This account contains a specification of different arms, viz. pikes, bows, cross-bows, arrows, guns and gun-powder. The following articles particularly fixed his attention:

1. One gun. 2. One gun and one charge of lead. 3. One gun and one charge of lead. 4. Two guns and two charges of lead. 5. One gun and one charge of lead. 6. Two guns and three charges of lead. Besides these articles, the following charge was made in the account:

*Pro Pulveribus, 35 shillings.*

*Pro Pulveribus quos domini emerunt ad Pyxides, 3 shillings.*

*Pro duabus Patellis ad Pyxides, 3 shillings.*

\* From the German of Crell's Annales, v. xx. p. C,

Historical facts  
to determine the  
first invention of  
gunpowder, and  
its use in war.

If we consider that guns, powder, and lead, are here charged, it is obvious that by the word *pulvis*, gun-powder must be understood; and by *patellis*, guns must be meant. From these documents we are led to believe that guns and gun-powder were known already before the year 1378. And it is more than probable that they were not purchased that year, but had probably been used before that time. For guns were too expensive for single individuals and small towns at that time; and on that account, the place in which I met with the said documents was very probably provided with guns many years after the invention of them. That this must have been the case becomes obvious from the following observation: Achilles Gefner, the Historian of Augsburg, who wrote a Latin Chronicle at the beginning of the 16th century, says,\* “ Three large cannons were cast at Augsburg in the “ year 1378, the largest of which discharged a ball of 127 “ pounds; the second a ball of 70 pounds, and the third a “ ball of 50 pounds, at a distance of 1000 paces.”

Herman Corner, who lived at the end of the 14th century, relates that the inhabitants of Lubec assisted the Emperor Charles, who besieged the castle of Dannenberg, with 600 *armatis cum duabus machinis*. *Bombardae enim pro tunc non erant ita communes, uti nunc sunt*: From hence it is evident that the 600 machines were nothing but bombardae, or guns.

Another remarkable document the author of this paper met with was the sentence of death of Nicolaus the Bold, who supplied the enemy with two barrels of gun-powder, in the year 1372. In this sentence it is clearly expressed, that the gun-powder was made up of saltpetre and sulphur. In the same year, the Corporation of Augsburg ordered to have cast, twenty cannons of metal, at the great expence of fifty pence. These cannons were intended to be used against their neighbours, the Bavarians.

Petrarch, born 1304, states, in his work † published 1374.

GAV. *Habeo machinas, ingentia saxa torquentes.*

RAL. *Saxa torquere furiosum est.*

GAV. *Habeo machinas et ballistas innumeras.*

RAL. *Mirum nisi et glandes Aeneas quae flammis ejectis horison tone tru jacuntur. Non erat siles de coelo tonantes ira*

\* Annales Augsburgerises.

† De Remediis utriusque fortunae.

*Dei immortalis, homuncio? O credulitas juncta suberbiæ. De terra etiam tonuisset: Non immutabile fulmen ut Maro ait humana rabies immitata est, et quod e nubibus mitti solet, ligneo quidem, sed tartareo mittitur instrumento. Erat hæc pestis nuper, rara ut cum ingenti miraculo cerneretur, nunc ut rerum pessimarum dociles sunt animi, ita communis est ut unum quodlibet genus armorum.*

Historical facts to determine the first invention of gunpowder, and its use in war.

We shall be less surprised that cannons and guns were made of wood; even in the 15th century guns were bound with iron hoops.

In the year 1365, Margraff Frederick, of Meissen, attempted to storm the town and castle of Einben with slings, battering rams, and other machines, then made use of in besieging towns. Rothe, who mentions this in his Chronicle of Thuringen, farther relates that the Duke Albert was in possession of a gun which he himself used at the siege, for shooting into the works of the enemy. It was, say this author, the first gun ever seen in that country.

In another document is stated, that *anno Domini, millesimo tricentesimo sexagesimo, consistorium urbis Lubecensis in toto combustum est, per negligentiam illorum qui pulveras pro bombardis parabant.* The same fire is mentioned by Herman Corner, a native of Lubeck. His words, as taken from the Chronicle of Lubeck, are as follows: *Consistorium urbis Lubecensis incensum est, et combustum per negligentiam illorum qui pulveres pro bombardis, sive petrarias parabant, secundum Chronicam Lubecensem. Cum enim prædictas parassent locabant eos in quodam loco consistoris non caute custoditos ab igne. Pulveres ergo per incuriam nocte accensi, domum ipsam succenderunt, ad antequam extinguui potuissent, cum in cineres redegerunt.* Consequently gunpowder must have been prepared already at Lubeck about the year 1360.

In the year 1359, a war broke out between the kings of Castile and Arragonia; in which the latter made use of a large gun, with which he did much damage to the vessels of the king of Castile; for he shot down with it the masts and rigging, and killed many men by only two shots.

Peter Divacus commemorates, that in the year 1356 the inhabitants of Lyons in Brabant purchased 12 guns (*bombardæ*) which were called thundering guns, or blunderbusses, *ab horrendo fragore.*



## V.

*A Chemical Analysis of some Calamines.* By JAMES SMITHSON, Esq. F. R. S. P. R. S. *From the Philosophical Transactions for 1803.*

Researches of Bergman on Calamines.

NOTWITHSTANDING the experiments of Bergman and others, on those ores of zinc which are called calamine, much uncertainty still subsisted on the subject of them. Their constitution was far from decided, nor was it even determined whether all calamines were of the same species, or whether there were several kinds of them.

Haüy's opinion that they are pure oxides.

The Abbé Haüy, so justly celebrated for his great knowledge in crystallography and mineralogy, has adhered, in his late work \*, to the opinions he had before advanced †, that calamines were all of one species, and contained no carbonic acid, being a simple calx of zinc, attributing the effervescence which he found some of them to produce with acids, to an accidental admixture of carbonate of lime.

Experiments.

The following experiments were made to obtain a more certain knowledge of these ores; and their results will show the necessity there was for their farther investigation, and how wide from the truth have been the opinions adopted concerning them.

*Calamine from Bleyberg.*

Calamine from Bleyberg.

a. The specimen which furnished the subject of this article, was said by the German of whom it was purchased, to have come from the mines of Bleyberg in Carinthia.

External characters.

It was in the form of a sheet stalactite, spread over small fragments of limestone. It was not however at all crystalline, but of the dull earthy appearance of chalk, though, on comparison, of a finer grain and closer texture.

It was quite white, perfectly opaque, and adhered to the tongue; 68.0 grs. of it, in small bits, immersed in distilled water, absorbed 19.8 grs. of it, = 0.29.

It admitted of being scraped by the nail, though with some difficulty: scraped with a knife, it afforded no light.

\* *Traité de Mineralogie*, Tome IV. † *Journal des Mines*. No. 32.

68.1 grs. of it, broken into small pieces, expelled 19.0 grs. Specific gravity, of distilled water from a stopple bottle. Hence its density = 3.584. In another trial, 18.96 grs. at a heat of 65° Fahrenheit, displaced 5.27 grs. of distilled water; hence the density = 3.598. The bits, in both cases, were entirely penetrated with water.

b. Subjected to the action of the blowpipe on the coal, it became yellow the moment it was heated, but recovered its pristine whiteness on being let cool. This quality, of temporarily changing their colour by heat, is common to most, if not all, metallic oxides; the white growing yellow, the yellow red, the red black.

Urged with the blue flame, it became extremely friable; spread yellow flowers on the coal; and, on continuing the fire no very long time, entirely exhaled. If the flame was directed against the flowers, which had settled on the coal, they shone with a vivid light. A bit fixed to the end of a slip of glass, wasted nearly as quickly as on the coal.

It dissolved in borax and microcosmic salt, with a slight effervescence, and yielded clear colourless glasses; but which became opaque on cooling, if over saturated. Carbonate of soda had not any action on it.

c. 68.0 grs. of this calamine dissolved in dilute vitriolic acid with a brisk effervescence, and emitted 9.2 grs. of carbonic acid. The solution was white and turbid, and on standing deposited a white powder, which, collected on a small filter of gauze paper, and welledulcorated and let dry, weighed only 0.86 grs. This sediment, tried at the blowpipe, melted first into an opaque white matter, and then partially reduced into lead. It was therefore, probably, a mixture of vitriol of lead and vitriol of lime.

Solutions in the sulphuric and muriatic acids gave only salts of zinc, and gave carbonic acid.

The filtered solution, gently exhaled to dryness, and kept over a spirit-lamp till the water of crystallization of the salt and all superfluous vitriolic acid were driven off, afforded 96.7 grs. of perfectly dry, or *arid*\*, white salt. On re-solution in water, and crystallization, this saline matter proved to be wholly vitriol

\* *Dry*, as opposed to wet or damp, which are only degrees of each other, merely implies free from mechanically admixed water. *Arid*, may be appropriated to express the state of being devoid of combined water.

of zinc, excepting an inappreciable quantity of vitriol of lime in capillary crystals, due, without doubt, to a slight and accidental admixture of some portion of the calcareous fragments on which this calamine had been deposited. Pure martial prussiate of tartar, threw down a white precipitate from the solution of this salt.

In another experiment, 20.0 grs. of this calamine afforded 28.7 grs. of arid vitriol of zinc.

d. 10 grs. of this calamine were dissolved in pure marine acid, with heat. On cooling, small capillary crystals of muriate of lead formed in the solution. This solution was precipitated by carbonate of soda, and the filtered liquor let exhale slowly in the air; but it furnished only crystals of muriate of soda.

e. 10 grs. dissolved in acetous acid without leaving any residuum. By gentle evaporation, 20.3 grs. = 2.03. of acetite of zinc, in the usual hexagonal plates, were obtained. These crystals were permanent in the air, and no other kind of salt could be perceived amongst them.

Neither solution of vitriolated tartar, nor vitriolic acid, occasioned the slightest turbidness in the solution of these crystals, either immediately or on standing; a proof that the quantity of lime and lead in this solution, if any, was excessively minute.

f. A bit of this calamine, weighing 20.6 grs. being made red hot in a covered tobacco-pipe, became very brittle, dividing on the slightest touch into prisms, like those of starch, and lost 5.9 grs. of its weight = 0.286. After this, it dissolved slowly and difficultly in vitriolic acid, without any effervescence.

The calamine contained oxide of zinc, carbonic acid, and water.

According to these experiments, this calamine consists of,

Calx of zinc	-	-	-	0.714
Carbonic acid	■	-	-	0.135
Water	-	-	-	0.151

1.000.

The carbonates of lime and lead in it are more accidental admixtures, and in too small quantity to deserve notice.

#### *Calamine from Somersetshire.*

Somersetshire calamine.

a. This calamine came from Mendip Hills in Somersetshire.

It had a mammillated form; was of a dense crystalline texture; semitransparent at its edges, and in its small fragments; and upon the whole very similar, in its general appearance, to calcedony.

It

It was tinged, exteriorly, brown ; but its interior colour was a greenish yellow.

It had considerable hardness ; it admitted however of being Characters. scraped by a knife to a white powder.

56.8 grs. of it displaced 13.1 grs. of water, at a temperature of 65° Fahrenheit. Hence its density = 4.336.

b. Exposed to the blowpipe, it became opaque, more yellow, Blowpipe assays. and friable ; spread flowers on the coal, and consequently volatilized, but not with the rapidity of the foregoing kind from Bleyberg.

It dissolved in borax and microcosmic salt, with effervescence, yielding colourless glasses. Carbonate of soda had no action on it.

c. It dissolved in vitriolic acid with a brisk effervescence ; Solution. and 67.9 grs. of it emitted 24.5 grs. = 0.360, of carbonic acid. This solution was colourless ; and no residuum was left. By evaporation, it afforded only vitriol of zinc, in pure limpid crystals.

d. 23.0 grs. in small bits, made red hot in a covered tobacco-pipe, lost 8.1 grs. = 0.352. It then dissolved slowly and difficultly in vitriolic acid, without any emission of carbonic acid ; and, on gently exhaling the solution, and heating the salt obtained, till the expulsion of all superabundant vitriolic acid and all water, 29.8 grs. of anhydrous vitriol of zinc were obtained. This dry salt was wholly soluble again in water ; and solution of pure martial prussiate of soda occasioned a white precipitate in it.

This calamine hence consists of,

Carbonic acid	-	-	-	0.352
Calx of zinc	-	-	-	0.648
				<hr/> 1.000.

Component parts, Carbonic acid and oxide of zinc.

#### *Calamine from Derbyshire.*

a. This calamine consisted of a number of small crystals, Derbyshire calamine. about the size of tobacco-seeds, of a pale yellow colour, which appeared from the shape of the mass of them, to have been deposited on the surface of crystals of carbonate of lime, of the form of Fig. 28. Plate IV. of the *Cristallographie* of Romé de L'Isle.

The smallness of these calamine crystals, and a want of External characters, &c. sharpness rendered it impossible to determine their form with certainty :

certainly; they were evidently, however, rhomboids, whose faces were very nearly, if not quite, rectangular, and which were incomplete along their six intermediate edges, apparently like Fig. 78. Plate IV. of Romé de L'Isle.

22.1 grs. of these crystals, at a heat of  $57^{\circ}$  Fahrenheit, displaced 5.1 grs. of water, which gives their density  $\approx 4.333$ .

Heat did not excite any electricity in these crystals.

#### Experiments.

b. Before the blowpipe, they grew more yellow and opaque, and spread flowers on the coal. They dissolved wholly in borax and microcosmic salt, with effervescence.

c. 22.0 grs. during their solution in vitriolic acid, effervesced, and lost 7.8 grs. of carbonic acid  $= 0.354$ . This solution was colourless, and afforded 26.8 grs. of arid vitriol of zinc, which, redissolved in water, shot wholly into clear colourless prisms of this salt.

Component  
parts carbonic  
acid and oxide  
of zinc.

d. 9.2 grs. of these crystals, ignited in a covered tobacco-pipe, lost 3.2 grs.  $= 0.3478$ ; hence these crystals consist of,

Carbonic acid	-	-	-	-	0.348
Calx of zinc	-	-	-	-	0.652
					<hr/> 1.000.

#### Electrical Calamine.

Electrical cal-  
amine from  
Regbania.

The Abbé Haüy has considered this kind as differing from the other calamines only in the circumstance of being in distinct crystals; but it has already appeared, in the instance of the Derbyshire calamine, that all crystals of calamine are not electric by heat, and hence, that it is not merely to being in this state that this species owes the above quality. And the following experiments, on some crystals of electric calamine from Regbania in Hungary, can leave no doubt of its being a combination of calx of zinc with quartz; since the quantity of quartz obtained, and the perfect regularity and transparency of these crystals make it impossible to suppose it a foreign admixture in them.

a. 23.45 grs. of these Regbania crystals, displaced 6.8 grs. of distilled water, from a stopple-bottle, at the temperature of  $64^{\circ}$  Fahrenheit; their specific gravity is therefore  $= 3.434$ .

The form of these crystals is represented in Figure I. Plate V. where the angle formed by the planes *a* and *c* was  $90^{\circ}$ , that by *a* and *e*  $= 150^{\circ}$ , that by *b* and *c*  $= 115^{\circ}$ , and that by *c* and *d*  $= 130^{\circ}$ .

They were not scratched by a pin; a knife marked them.

b. One of these crystals, exposed to the flame of the blow-pipe, decrepitated and became opaque, and shone with a green light, but seemed totally infusible. Blowpipe experiments.

Borax and microcosmic salt dissolved these crystals, without any effervescence, producing clear colourless glasses, Carbonate of soda had little if any action on them.

c. According to Mr. Pelletier's experiments \* on the calamine of Fribourg in Brisgaw, which is undoubtedly of this species, its composition is,

Quartz	-	-	-	-	-	0.50
Calx of zinc	-	-	-	-	-	0.38
Water	-	-	-	-	-	0.12
						1.00.

The experiments on the Reghania crystals have had different results; but, though made on much smaller quantities, they will perhaps not be found, on repetition, less in conformity with nature.

23.45 grs. heated red hot in a covered crucible, decrepitated a little, and became opaque, and lost 1.05 grs. but did not fall to powder or grow friable. It was found, that this matter was not in the least deprived of its electrical quality by being ignited; and hence, while hot, the fragments of these decrepitated crystals clung together, and to the crucible.

d. 22.2 grs. of these decrepitated crystals, = 23.24 grs. of the original crystals, in a state of impalpable powder, being digested over a spirit-lamp with diluted vitriolic acid, showed no effervescence; and, after some time, the mixture became a jelly. Exhausted to dryness, and ignited slightly, to expel the superfluous vitriolic acid, the mass weighed 37.5 grs.

On extraction of the saline part by distilled water, a fine powder remained, which, after ignition, weighed 5.8 grs. and was quartz.

The saline solution afforded, on crystallization, only vitriol of zinc. These crystals therefore consist of, Component parts of electrical calamine.

Quartz	-	-	-	-	-	0.250
Calx of zinc	-	-	-	-	-	0.683
Water	-	-	-	-	-	0.044
						0.977
Loss	-	-	-	-	-	0.023

1.000.

The water is most probably not an essential element of this calamine, or in it in the state of, what is improperly called, water of crystallization, but rather exists in the crystals in fluid drops interposed between their plates, as it often is in crystals of nitre, of quartz, &c. Its small quantity, and the crystals not falling to powder on its expulsion, but retaining almost perfectly their original solidity, and spathose appearance in the places of fracture, and, above all, preserving their electrical quality wholly unimpaired, which would hardly be the case after the loss of a real element of their constitution, seem to warrant this opinion.

If the water is only accidental in this calamine, its composition, from the above experiments, will be,

Quartz	-	-	-	-	-	0.261
Calx of zinc	-	-	-	-	-	0.739
						<hr/> 1.000

It is found in  
Derbyshire.

I have found this species of calamine amongst the productions of Derbyshire, in small brown crystals, deposited, together with the foregoing small crystals of carbonate of zinc, on crystals of carbonate of lime. Their form seems, as far as their minuteness and compression together would allow of judging, nearly or quite the same as that of those from Reg-bania; and the least atom of them immediately evinces its nature, on being heated, by the strong electricity it acquires. On their solution in acids, they leave quartz.

### OBSERVATIONS.

Chemistry is yet so new a science, what we know of it bears so small a proportion to what we are ignorant of, our knowledge in every department of it is so incomplete, so broken, consisting so entirely of isolated points thinly scattered like lucid specks on a vast field of darkness, that no researches can be undertaken without producing some facts, leading to some consequences, which extend beyond the boundaries of their immediate object.

Component  
parts of sulphate  
of zinc.

1. The foregoing experiments throw light on the proportions in which its elements exist in vitriol of zinc. 23.0 grs. of the Mendip Hill calamine, produced 29.8 grs. of arid vitriol of zinc. These 23.0 grs. of calamine contained 14.9 grs. of calx

calx of zinc; hence, this metallic salt, in an arid state, consists of *exactly equal* parts of calx of zinc and vitriolic acid.

This inference is corroborated by the results of the other experiments: 68.0 grs. of the Bleyberg calamine, containing 48.6 grs. of calx of zinc, yielded 96.7 grs. of arid vitriol of zinc; and, in another trial, 20.0 grs. of this ore, containing 14.2 grs. of calx of zinc, produced 28.7 grs. of arid vitriol of zinc. The mean of these two cases, is 62.7 grs. of arid vitriol of zinc, from 31.4 grs. of calx of zinc.

In the experiment with the crystals of carbonate of zinc from Derbyshire, 14.35 grs. of calx of zinc furnished indeed only 26.8 grs of arid vitriol of zinc; a deficiency of about  $\frac{6}{100}$ , occasioned probably by some small inaccuracy of manipulation.

2. When the simplicity found in all those parts of nature which are sufficiently known to discover it is considered, it appears improbable that the proximate constituent parts of bodies should be united in them, in the very remote relations to each other in which analyses generally indicate them; and, an attention to the subject has led me to the opinion that such is in fact not the case, but that, on the contrary, they are universally, as appears here with respect to arid vitriol of zinc, fractions of the compound of very low denominators. Possibly in few cases exceeding five.

Position that the parts of compounds do not greatly exceed each other in quantity.

The success which has appeared to attend some attempts to apply this theory, and amongst others, to the compositions of some of the substances above analysed, and especially to the calamine from Bleyberg, induces me to venture to dwell here a little on this subject, and state the composition of this calamine, which results from the system, as, besides contributing perhaps to throw some light on the true nature of this ore, it may be the means likewise of presenting the theory under circumstances of agreement with experiment, which, from the surprising degree of nearness, and the trying complexity of the case, may seem to entitle it to some attention.

From this calamine, containing, according to the results of the experiments on the Mendip Hill kind, too small a quantity of carbonic acid to saturate the whole of the calx of zinc in it, and from its containing much too large a portion of water to be in it in the state of mere moisture or dampness, it seems to consist of two matters; carbonic of zinc, and a peculiar compound of zinc and water, which may be named *hydrate of zinc*.

Hence the component parts of Bleyberg calamine are supposed to be arranged in subordinate compounds.



By the results of the analysis of the Mendip-Hill calamine, corrected by the theory, carbonate of zinc appears to consist of,

Carbonic acid	-	-	-	-	$\frac{2}{3}$
Calx of zinc	-	-	-	-	$\frac{2}{3}$

Deducting from the calx of zinc in the Bleyberg calamine, that portion which corresponds, on these principles, to its yield of carbonic acid, the remaining quantity of calx of zinc and water are in such proportions as to lead, from the theory, to consider hydrate of zinc as composed of

Calx of zinc	-	-	-	-	$\frac{2}{3}$
Water, or rather ice	-	-	-	-	$\frac{1}{3}$

And, from these results, corrected by the theory, I consider Bleyberg calamine as consisting of,

Carbonic of zinc	-	-	-	-	$\frac{2}{3}$
Hydrate of zinc	-	-	-	-	$\frac{1}{3}$

The test of this hypothesis is in the quantities of the remote elements which analysis would obtain from a calamine thus composed.

The following table will show how very insignificantly the calamine compounded by the theory, would differ in this respect from the calamine of nature.

Elucidations of chemical theory. 1000 parts of the compound salt of carbonate and hydrate of zinc consist of,

Carbonate of zinc 400 - - - - =	{	Carbonic acid	= $\frac{400}{3}$	= - - - - -	$133\frac{1}{3}$	}	= - 716 $\frac{2}{3}$
		Calx of zinc	= $\frac{400 \times 2}{3}$	= 266 $\frac{2}{3}$			
Hydrate of zinc = 600 - - - -	{	Calx of zinc	= $\frac{600 \times 3}{4}$	= 450			
		Ice - -	= $\frac{600}{4}$	= - - - - -	150		
						<hr/> 1000.	

Great as is the agreement between the quantities of the last column and those obtained by the analysis of the Bleyberg calamine, it would be yet more perfect, probably, had there been, in this instance, no sources of fallacy but those attached to chemical operations, such as errors of weighing, waste, &c. but

but the differences which exist are owing, in some measure at least, to the admixture of carbonate of lime and carbonate of lead, in the calamine analysed, and also to some portion of water, which is undoubtedly contained, in the state of moisture, in so porous and bibulous a body.

It has also appeared, in the experiments on the Mendip Hill calamine, that acids indicate a greater quantity of carbonic acid than fire does,  $\frac{1823}{1000}$ . If we make this deduction for dissolved water, it reduces the quantity of carbonic acid in the Bleyberg calamine, to 0.1321.

If we assume this quantity of carbonic acid as the datum to calculate, on this system, the composition of the calamine from Bleyberg, we shall obtain the following results :

Compound salt, of carbonate of zinc and hydrate of zinc	990.3
Water in the state of moisture	2.5
Carbonate of lime and carbonate of lead	7.2
	<hr/>
	1000.0

It may be thought some corroboration of the system here offered, that, if we admit the proportions which it indicates, the remote elements of this ore, while they are regular parts of their immediate products, by whose subsequent union this ore is engendered, are also regular fractions of the ore itself: thus,

The carbonic acid	-	-	-	= $\frac{8}{25}$
The water	-	-	-	= $\frac{2}{25}$
The calx of zinc	-	-	-	= $\frac{4}{25}$

Hereby displaying that sort of regularity, in every point of view of the object, which so wonderfully characterises the works of nature, when beheld in their true light.

If this calamine does consist of carbonate of zinc and hydrate of zinc, in the regular proportions above supposed, little doubt can exist of its being a true chemical combination of these two matters, and not merely a mechanical mixture of them in a pulverulent state; and, if so, we may indulge the hope of some day meeting with this ore in regular crystals.

If the theory here advanced has any foundation in truth, the discovery will introduce a degree of rigorous accuracy and certainty into chemistry, of which this science was thought to be ever incapable, by enabling the chemist, like the geometer, to rectify by calculation the unavoidable errors of his

Elucidations of manual operations, and by authorising him to eliminate from chemical theory. the essential elements of a compound, those products of its analysis whose quantity cannot be reduced to any admissible proportion.

A certain knowledge of the exact proportions of the constituent principles of bodies, may likewise open to our view harmonious analogies between the constitutions of related objects, general laws, &c. which at present totally escape us. In short, if it is founded in truth, its enabling the application of mathematics to chemistry, cannot but be productive of material results \*.

3. By the application of the foregoing theory to the experiments on the electrical calamine, its elements will appear to be,

Quartz	-	-	-	-	-	$\frac{1}{4}$
Calx of zinc	-	-	-	-	-	$\frac{3}{4}$

A small quantity of the calamine having escaped the action of the vitriolic acid, and remained undecomposed, will account for the slight excess in the weight of the quartz.

4. The exhalation of these calamines at the blowpipe, and the flowers which they diffuse round them on the coal, are probably not to be attributed to a direct volatilization of them. It is more probable that they are the consequences of the dis-oxidation of the zinc calx, by the coal and the inflammable matter of the flame, its sublimation in a metallic state, and instantaneous recalcination. And this alternate reduction and combustion, may explain the peculiar phosphoric appearance exhibited by calces of zinc at the blowpipe.

The apparent sublimation of the common flowers of zinc at the instant of their production, though totally unsublimable afterwards, is certainly likewise but a deceptive appearance. The reguline zinc, vaporised by the heat, rises from the crucible as a metallic gas, and is, while in this state, converted to a calx. The flame which attends the process is a proof of it; for flame is a mass of vapour, ignited by the production of fire within itself. The fibrous form of the flowers of zinc, is owing to a crystallization of the calx while in *mechanical sus-*

\* It may be proper to say, that the experiments have been stated *precisely* as they turned out, and have not been in the *least degree* bent to the system.

*pen* in the air like that which takes place with camphor' Elucidations of chemical theory.  
when, after having been some time inflamed, it is blown out.

A moment's reflection must evince, how injudicious is the common opinion, of crystallization requiring a state of solution in the matter; since it must be evident, that while solution subsists, as long as a quantity of fluid admitting of it is present, no crystallization can take place. The only requisite for this operation, is a freedom of motion in the masses which tend to unite, which allows them to yield to the impulse which propels them together, and to obey that sort of polarity which occasions them to present to each other the parts adapted to mutual union. No state so completely affords these conditions as that of mechanical suspension in a fluid whose density is so great, relatively to their size, as to oppose such resistance to their descent in it as to occasion their mutual attraction to become a power superior to their force of gravitation. It is in these circumstances that the atoms of matters find themselves, when, on the separation from them of the portion of fluid by which they were dissolved, they are abandoned in a disengaged state in the bosom of a solution; and hence it is in saturated solutions sustaining evaporation, or equivalent cooling, and free from any perturbing motion, that regular crystallization is usually effected.

But those who are familiar with chemical operations, know the sort of agglutination which happens between the particles of subsided very fine precipitates: occasioning them, on a second diffusion through the fluid, to settle again much more quickly than before, and which is certainly a crystallization, but under circumstances very unfavourable to its perfect performance.

5. No calamine has yet occurred to me which was a real, uncombined, calx of zinc. If such, as a native product, should ever be met with in any of the still unexplored parts of the earth, or exist amongst the unscrutinized possessions of any cabinet, it will easily be known, by producing a quantity of arid vitriol of zinc exactly double its own weight; while the hydrate of zinc, should it be found single, or uncombined with the carbonate, will yield, it is evident, 1.5 its weight of this arid salt,

## VI.

*Table of the Radii of Wheels, from Ten to Three Hundred Teeth, the Pitch\* being Two Inches. By Mr. B. DONKIN, Millwright, Dartford, Kent †.*

Table of the radii of wheels.

No. of Teeth	Radius in Inches.	No.	Radius.	No.	Radius.
10	3,236	42	13,382	74	23,562
11	3,549	43	13,700	75	23,880
12	3,864	44	14,018	76	24,198
13	4,179	45	14,336	77	24,517
14	4,494	46	14,654	78	24,835
15	4,810	47	14,972	79	25,153
16	5,126	48	15,290	80	25,471
17	5,442	49	15,608	81	25,790
18	5,759	50	15,926	82	26,108
19	6,076	51	16,244	83	26,426
20	6,392	52	16,562	84	26,744
21	6,710	53	16,880	85	27,063
22	7,027	54	17,198	86	27,381
23	7,344	55	17,517	87	27,699
24	7,661	56	17,835	88	28,017
25	7,979	57	18,153	89	28,336
26	8,296	58	18,471	90	28,654
27	8,614	59	18,789	91	28,972
28	8,931	60	19,107	92	29,290
29	9,249	61	19,425	93	29,608
30	9,567	62	19,744	94	29,927
31	9,885	63	20,062	95	30,245
32	10,202	64	20,380	96	30,563
33	10,520	65	20,698	97	30,881
34	10,838	66	21,016	98	31,200
35	11,156	67	21,335	99	31,518
36	11,474	68	21,653	100	31,836
37	11,792	69	21,971	101	32,155
38	12,110	70	22,289	102	32,473
39	12,428	71	22,607	103	32,791
40	12,746	72	22,926	104	33,109
41	13,064	73	23,244	105	33,427

\* By the pitch is understood the distance between the centers of two contiguous teeth; and by the radius is understood the distance between the center of the wheel and the center of each tooth.

† Communicated by the author.

# RADII OF WHEELS.

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Table of the  
radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
106	33,746	151	48,068	196	62,392
107	34,064	152	48,387	197	62,710
108	34,382	153	48,705	198	63,028
109	34,700	154	49,023	199	63,346
110	35,018	155	49,341	200	63,665
111	35,337	156	49,660	201	63,983
112	35,655	157	49,978	202	64,301
113	35,974	158	50,296	203	64,620
114	36,292	159	50,615	204	64,938
115	36,611	160	50,933	205	65,256
116	36,929	161	51,251	206	65,574
117	37,247	162	51,569	207	65,893
118	37,565	163	51,888	208	66,211
119	37,883	164	52,206	209	66,529
120	38,202	165	52,524	210	66,848
121	38,520	166	52,843	211	67,166
122	38,838	167	53,161	212	67,484
123	39,156	168	53,479	213	67,803
124	39,475	169	53,798	214	68,121
125	39,793	170	54,116	215	68,439
126	40,111	171	54,434	216	68,757
127	40,429	172	54,752	217	69,075
128	40,748	173	55,071	218	69,394
129	41,066	174	55,389	219	69,712
130	41,384	175	55,707	220	70,031
131	41,703	176	56,026	221	70,349
132	42,021	177	56,344	222	70,667
133	42,339	178	56,662	223	70,985
134	42,657	179	56,980	224	71,304
135	42,976	180	57,299	225	71,622
136	43,294	181	57,617	226	71,941
137	43,612	182	57,935	227	72,258
138	43,931	183	58,253	228	72,577
139	44,249	184	58,572	229	72,895
140	44,567	185	58,890	230	73,214
141	44,885	186	59,209	231	73,532
142	45,204	187	59,527	232	73,850
143	45,522	188	59,845	233	74,168
144	45,840	189	60,163	234	74,487
145	46,158	190	60,482	235	74,805
146	46,477	191	60,800	236	75,123
147	46,795	192	61,118	237	75,441
148	47,113	193	61,436	238	75,760
149	47,432	194	61,755	239	76,078
150	47,750	195	62,073	240	76,397

No.

Table of the  
radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
241	76,715	261	83,081	281	89,447
242	77,033	262	83,399	282	89,765
243	77,351	263	83,717	283	90,084
244	77,670	264	84,036	284	90,402
245	77,988	265	84,354	285	90,720
246	78,306	266	84,673	286	91,038
247	78,625	267	84,991	287	91,357
248	78,943	268	85,309	288	91,675
249	79,261	269	85,627	289	91,993
250	79,580	270	85,946	290	92,312
251	79,898	271	86,264	291	92,630
252	80,216	272	86,582	292	92,948
253	80,534	273	86,900	293	93,267
254	80,853	274	87,219	294	93,585
255	81,171	275	87,537	295	93,903
256	81,489	276	87,855	296	94,222
257	81,808	277	88,174	297	94,540
258	82,126	278	88,492	298	94,858
259	82,444	279	88,810	299	95,177
260	82,763	280	89,129	300	95,495

*N. B.* When the pitch is different from two inches, the radius of a wheel of any number of teeth, from 10 to 300 may be found from this table, by the Rule of Three; for as two inches (the pitch in the table) is to any radius in the table, so is any given pitch to the radius required.

For Example; let it be required to find the radius of a wheel of 100 teeth, when the pitch is  $1\frac{1}{4}$  inches. The radius of a wheel of 100 teeth is, in the table, 31,836 inches. Accordingly we have  $2 : 31,836 :: 1,25$ , to the number of inches in the radius required; which will be found 19,897 as by the Operation annexed.

$$2 : 31,836 :: 1,25$$

$$\begin{array}{r} 159180 \\ 63672 \\ \hline 31836 \end{array}$$

$$\begin{array}{r} 2) 39,79500 \\ \hline 19,8975 \end{array}$$

## VII.

*Account of the Pyrometer of Platina. By CITIZEN GUYTON.\**

CITIZEN GUYTON presented an instrument to the sitting of the French National Institute of the 26th Floreal last, intended to measure the highest degrees of heat of our furnaces. It consists of a rod or plate of platina placed horizontally in a groove formed in a cake of hardened white clay. This plate is supported at one of its extremities on the part of the mass which terminates the groove; the other end presses against a bended lever, whose longest arm forms an index to a graduated arc; so that the change of position of this index indicates the expansion produced on the plate of metal by the heat.

*Pyrometer for red heats, consisting of a lever of platina moved by the expansion of a bar of the same metal; the whole being supported on a mass of baked clay.*

*It is governed by the difference of the expansions of pottery and platina.*

*Description.*

The cake of clay having been highly baked, leaves no cause to apprehend any contraction; and the expansion which may take place during the ignition will only affect the very small distance between the axis of motion of the index and the point of contact of the plate, that is to say, in such a manner as rather to diminish the effect than to increase it.

All the parts of this instrument being of platina, neither fusion nor oxidation are to be apprehended.

With respect to its dimensions, the author conceives that in order to render the use of it commodious and accurate, they should be reduced to such as may be necessary to obtain sensible variations; it will then be rendered commodious by the facility with which it may be placed under a muffle or an inverted crucible, &c. and accurate, because the probabilities of any accidental inequalities of the heat will be diminished, which it is impossible to avoid to a certain extent, even in the midst of a large mass of fire.

The variations will be sufficiently perceptible, if we can not only estimate, but correctly determine expansions of the part of a millimetre (about the 5000th part of an inch,) these the author obtains by the proportions of the instrument which he has himself adopted.

The rod or plate of expansion is 45 millimetres (one inch and three quarters) in length, 5 in width (one fifth of an inch) and 2 in thickness (one thirteenth of an inch.)



The arm of the bended lever, which presses against the end of this rod, is 25 millimetres in length; (rather  $2\frac{1}{2}$ , or about one ninth of an inch) and the arm at right angles to it, or the index, which traverses on the graduated arc, is 50 millimetres in length (one inch and eight tenths) or twenty times the length of the other. The space traversed by the displacing of the small arm will be thus encreased in the proportion of 1 to 20.

As the long arm or index carries a nonius which divides each degree on the graduated arc into ten parts, we can distinctly observe the 200th of one of those measures (referred to the bar itself.)

Lastly, As the decimal division of an arc of a circle of 50 millimetres radius, gives only 7.8538 deci-millimetres for one of its degrees, it is evident then that we may measure an expansion of 0.078538 deci-millimetres, or of the 5730th part of the length of the radius.

In order to prevent the position of the index from being changed in removing the instrument from the furnace, a plate of platina is fixed so as to form a spring against its extremity.

The author has commenced a series of experiments to determine the range of this pyrometer, to compare it with the pyrometric pieces of Wedgewood, and so to shew the degree of confidence it merits, the methods of using, and the cases in which it may be usefully employed in philosophical researches and in the arts.

## VIII.

*Letter from Mr. Ezekiel Walker on the Proportion of Light  
afforded by Candles of different Dimensions.*

To Mr. NICHOLSON,

SIR,

Observations on  
a letter to the  
editor.

YOUR correspondent, who has made some remarks on my experiments on candles, does not seem to have sufficiently considered his subject; for had he paid attention to my paper on page 40 of the fourth volume of your Journal, he could not have advanced that "Though Mr. Walker asserts with considerable decision, that the light afforded by candles, is proportioned to the quantity of material consumed, yet he has not given

given the detail of his experiments, but seems in some measure to have discovered this result by argument, from the supposed nature of the subject." \*

In the table in my paper above mentioned, the last column contains the distances of the candles from the wall, when the shadows were equal; and the fourth column contains the weights of those candles consumed in a given time, and these are all the data required for making the calculations, to show whether my deduction is true or false. The mode of calculating seemed to me, at the time I wrote that paper, too easy to need any illustration, but as I now stand charged by your correspondent, of having deduced a general law from doubtful principles, a further explanation becomes necessary.

Observations of light emitted by candles, and of the weights consumed.

To investigate rules for this purpose, 1. Let  $M$  represent the mould candle,  $a$  its distance from the wall, on which the shadows were compared,  $x$  its quantity of matter consumed in a given time, ( $t$ ) and  $Q$  the quantity of light emitted by  $M$  in the same time: 2. Let  $m$  represent any other candle,  $b$  its distance from the same wall, and  $y$  its quantity of matter consumed, in the time  $t$ .

Investigation of the rules for computing.

Then as the intensities of light are directly as the squares of the distances of the two candles from the wall, we have, as  $a^2 : Q :: b^2 : \frac{b^2 \times Q}{a^2}$  = the quantity of light, emitted by  $m$  in the time.

Then let us suppose that the quantities of light are directly as the quantities of matter consumed in the time  $t$ , and we have, As  $x : Q :: y : \frac{y \times Q}{x}$  = the quantity of light emitted by  $m$  in that time, by hypothesis.

Now, when  $\frac{b^2 \times Q}{a^2}$  (Theo. 1.) is  $= \frac{Y \times Q}{X}$  (Theo. 2.) the quantities of light of  $M$  and  $m$  are directly as their quantities of matter consumed in any given time.

By these rules, the calculations contained in the following table, were made from the experiments mentioned at the beginning of this paper.

\* See Philosophical Journal, VOL. V. page 219.

No of experiments.		Light by Rule I.	Light by Rule II.	2d Rule differs from the 1st.
1	No. 1. compd. with the mould	1.000	1.000	.000
2	No. 1. compd. with do.	1.000	1.000	.000
3	No. 1. compd. with do.	1.000	1.015	+ .015
3	No. 3. compd. with do.	1.196	1.125	- .071
4	No. 4. compd. with do.	1.196	1.226	+ .030
The mean error of the 2d Rule - .005				

As the mean result given by the 2d rule, differs only 1 in 200 from the 1st, which is universally received as true, the 2d rule appears sufficiently exact for many practical purposes, where the properties of that light is concerned, which is produced by candles.

EZEKIEL WALKER.

*Lynn Regis, 20th Sept. 1803.*

Whether the experiments of a correspondent be accurate.

P. S. As to your correspondent's experiment, it does not appear so correct to me, as it appears to himself; for every one knows, that one end of a mould candle is thicker than the other, therefore if that gentleman made his experiment with the small end of his candle, he has estimated the quantity of light produced by a pound, too little; and if he made his experiment with the large end, his estimation is too great; and moreover, it may be doubted, whether the  $\frac{1}{18}$  part of a pound of candles, can be so exactly ascertained by measuring as by weighing, even if the candles were perfect cylinders.

## IX.

*On the Compounds of Sulphur and Oxygen. By THOMAS THOMSON, M. D, Lecturer on Chemistry in Edinburgh. From the Author.*

Three known compounds of sulphur and oxygen, 1. oxide, 2. acids.

IT is at present the opinion of Chemists that sulphur is capable of combining with three doses of oxygen, and of forming three distinct compounds, namely,

1. Oxide of Sulphur.
2. Sulphurous Acid.
3. Sulphuric Acid.

The

The first of these is supposed to contain a minimum, the third a maximum of oxygen. Of these three the constituents of the last only have been ascertained with precision. It will be proper to begin with it, as the knowledge of its composition may be of service in ascertaining the constituents of the rest.

### I. *Of Sulphuric Acid.*

This acid has been lately analysed with precision by Sulphuric acid, Thenard and Chenevix. I have repeated their experiments composed of 61 sulphur and 39 oxygen. with care, and have obtained for the mean result 39 per cent. of oxygen, which is only one half per cent. greater than the result obtained by Mr. Chenevix. This difference in the present state of analysis may be accounted altogether insignificant. I shall consider sulphuric acid, then, as composed of

$$\begin{array}{r} 61 \text{ sulphur} \\ 39 \text{ oxygen} \\ \hline 100 \end{array}$$

### II. *Of Sulphurous Acid.*

Most of the properties of this Acid have been long known Sulphurous acid. to chemists; but no experiments have been made to ascertain the proportion of its component parts. Before I proceed to relate the result of mine, it may be worth while to describe a few of the properties of Sulphurous Acid, which have not hitherto been stated with precision.

1. Fifty-three measures of sulphurous acid gas were introduced into a graduated tube standing over mercury, and one measure of water was thrown up. In five minutes 20 measures of gas were absorbed, and in 24 hours the absorption amounted to 33 measures. No farther absorption took place in three days more; but on introducing the tube into water, the whole gas disappeared, except a small globule, which did not exceed 1-10th of a measure. During this experiment the thermometer at the time of observation deviated very little from 61°, and the barometer oscillated from 29.55 to 29.77. Water then, at the temperature of 61°, absorbs 33 times its bulk of this gas. Now, if with Lavoisier, we suppose a cubic inch of gas to weigh 0.63 grs. a cubic inch of water will absorb 19.79 grains of sulphurous acid, and 100 parts of water will absorb 8.21 parts by weight.

Water at 60° absorbs nearly one-eleventh part of its weight of sulphurous gas.

2. A current

The impregnated water is intensely acid. Sp. gravity 1.0513. Slight heat disengages the acid.

2. A current of sulphurous acid gas was passed through a large quantity of water till the liquid refused to absorb any more. The taste of the water thus saturated, was intensely acid and sulphureous, and its odour excessively strong. The specific gravity at the temperature  $68^{\circ}$  was 1.0513; the heat of the hand was sufficient to occasion an extrication of gas. When moderately heated, it frothed violently, and exhaled the dense blue smoke which usually indicates the presence of sulphurous acid. When boiled down in a retort to half its bulk, it lost its smell, but still continued slightly acid. Hence it obviously contained sulphuric acid.

Analysis of sulphites. The solution of sulphurous acid contains a small portion of sulphuric acid.

3. In analysing the different sulphites, I have not found barytes answer so well as I was led to expect from the experiments of Fourcroy and Vauquelin; the solubility of sulphite of barytes in water is so considerable, that precision by means of it is scarcely to be looked for. But nitrate of lead yields with the alkaline and earthy sulphites a white insoluble powder of sulphite of lead, which may be dried in the temperature of  $300^{\circ}$  without decomposition, and is then composed of about

25 sulphurous acid.

75 yellow oxide of lead.

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100

One hundred parts of the above liquid sulphurous acid yielded, with nitrate of lead, a precipitate indicating the presence of 6.15 parts of sulphurous acid. Another hundred parts, boiled down to one half in a retort, yielded, with muriate of barytes, a precipitate indicating the presence of 0.34 sulphuric acid. Therefore, 100 parts of my liquid sulphurous acid contained about

5.81 sulphurous acid,

0.34 sulphuric acid,

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6.15.

So that the sulphuric acid amounts nearly to  $\frac{1}{16}$  of the sulphurous. The presence of this acid is a proof of an affinity between sulphurous acid gas and sulphuric acid; for the gas was passed through an intermediate vessel before it reached the water.

This contamination seems to render the water less capable of absorbing sulphurous gas.

4. The proportion of acid combined with water in the liquid sulphurous acid was rather less than 7; yet, when water is plunged into a large column of gas, we have seen that it absorbs

absorbs rather more than eight parts of it by weight. Perhaps this difference was owing to the presence of the sulphuric acid in the liquid. For water, slightly acidulated with sulphuric acid, absorbs a smaller proportion of gas, than pure water.

5. After trying various experiments, in order to ascertain the constituents of sulphurous acid, I found the following method most to be depended on.

Sulphite of potash was obtained by Berthollet's method. It is a fine white salt, the properties of which have been very fully detailed by Fourcroy and Vauquelin, though they have neglected to analyse it.

When this salt is exposed for a few minutes to a heat of  $300^{\circ}$ , it loses 3.3 per cent. of its weight; and suffers no additional loss, though the heat be continued for an hour. When heated to redness in a platinum crucible, it decrepitates, becomes of an opake white, a blue flame issues from below the lid, and, on taking off the cover at that instant, the salt may be observed of a glowing red heat in the middle. When this glow disappears, the salt will be found to have sustained a loss of 22.3. per cent. and it loses no more, though melted, and kept half an hour in fusion. On evolving, it splits into the fine thin transparent plates, which distinguish sulphate of potash in the same circumstances. When this residue is dissolved in water, and treated with muriate of barytes, this precipitate of sulphate of barytes obtained, when dried and heated to redness, weighs 95.5, indicating the presence of 22.92 sulphuric acid. Supposing with M. Chenevix, that sulphate of barytes contains 24 per cent. of sulphuric acid: hence it follows that sulphate of potash is composed of

22.30	volatile matter
22.25	sulphuric acid
55.45	potash

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100.00

When 100 grains of sulphate of potash were exposed to the heat of a lamp in a retort with a very long beak, fitted to a mercurial air holder, they decrepitated and assumed the appearance of an opake white powder: 18 cubic inches of gas were extricated, and sulphur, with a little water, was volatilized into the beak of the retort; the gas was absorbed by water, and had the usual smell of sulphurous acid: the retort had

had lost 15.2 grains of weight. The sulphur being carefully collected, was found to weigh 5.1 grains. When burnt, it left 0.1 of residuum, which seemed to be sulphurate of iron, for it gave a yellow colour to muriatic acid ;\* the water volatilized could not be weighed, but I estimate it at 2 grains. The experiment shews us what the volatile matter is which is drawn off when sulphate of potash is heated to redness. It is composed of

15.2	sulphurous acid
5.1	sulphur
2.0	water

---

22.3

The salt which remained in the retort being dissolved, and treated with muriate of barytes, gave a precipitate which indicated the presence of 23.2 of sulphuric acid. Hence sulphate of potash is composed of

Component parts  
of sulphate of  
potash.

23.2	sulphuric acid
15.2	sulphurous acid
5.1	sulphur
54.5	potash
2.0	water

---

100.0

But it is obvious that, before the application of heat, the first three constituents together constituted sulphurous acid. Hence sulphite of potash is composed of

43.5	sulphurous acid
54.5	potash
2.0	water

---

100.0

Explanation of  
the action of  
heat on sulphite  
of potash.

This analysis enables us to trace the changes produced upon sulphite of potash by heat. A temperature of 300° separates the water and a small portion of sulphurous acid, which seems more loosely combined; for the salt, in consequence, loses its smell; an increase of heat occasions a separation of a portion of the acid, unaltered; the remainder divides itself into two parts, namely, sulphuric acid, which remains combined with the potash, and sulphur, which sublimes. Hence

\* I have never yet burned sulphur, without observing traces of a similar residuum.

We learn, that sulphurous acid is composed of 23.2 sulphuric acid, and 5.1 sulphur, which gives us

$$\begin{array}{r} 82 \text{ sulphuric acid} \\ 18 \text{ sulphur} \\ \hline 100 \end{array}$$

But 100 parts of sulphuric acid contain 39 of oxygen; therefore 82 contain nearly 32. Hence sulphurous acid is composed of

$$\begin{array}{r} 68 \text{ sulphur} \\ 32 \text{ oxygen} \\ \hline 100 \end{array}$$

Fourcroy affirms, that sulphurous acid contains only about 15 per cent. of oxygen, which is less than one half of the result just given. But he quotes no experiment in proof of his assertion. In all probability it was a mere guess.

6. The phenomena which attend the acidification of sulphur and the decomposition of sulphurous acid, render it probable that sulphurous acid is rather a compound of sulphuric acid and sulphur, than of sulphur and oxygen. Sulphurous acid is most probably sulphur combined with sulphuric acid.

Sulphur and sulphuric acid combine with great facility. For if we form them into a probe, a very moderate heat is sufficient to convert the whole into sulphurous acid gas.

Whenever sulphur is acidified, a portion of sulphuric acid always makes its appearance in whatever way the process is conducted. Such at least has been the constant result of my experiments.

When sulphur is exposed to the heat of an Argand lamp in a retort connected with a mercurial air holder, it melts and sublimes at first rapidly, but much more slowly, when the process has continued for some time. In a retort, whose capacity was 63 cubic inches, four hours elapsed before  $\frac{1}{2}$  oz. of sulphur was sublimed into its neck. A considerable quantity of air was driven over; but on allowing the vessels to cool, the whole returned again, except 3 cubic inches. So that, by the operation, the air in the retort had increased about  $\frac{1}{10}$ th part. It smelt very pungently of sulphurous acid. When agitated in water, a small portion of it disappeared. The water did not acquire a perceptible taste, but it precipitated muriate of barytes even after being boiled for some time. A portion of this air, after being well washed, was left in contact with a stick of phosphorus over water. Its bulk was di-



minished 17 per cent. Hence it had lost 5 per cent. of oxygen by the action of the hot sulphur on it. Here we see the source of the acidification of the sulphur during its sublimation. From this experiment we are authorized to conclude that both sulphuric and sulphurous acids may be formed merely by heating sulphur in common air, without any sensible combustion.

Sulphur, when-  
ever sublimed, is  
in part acidified.

Sulphuric acid seems to be formed whenever sulphur is sublimed. For every specimen of flowers of sulphur which I have had an opportunity of examining, contained that acid. If common flowers of sulphur be boiled in water, the liquid always precipitates muriate of barytes. But flowers of sulphur, when once they have been well washed and dried, communicate no such property to water. If we now sublime these very flowers a second time, water in which they are boiled, precipitates muriate of barytes, as at first.

Combustion of  
sulphur under a  
jar over water.

When a red hot glass capsule is rapidly placed on a pedestal of standing water, sulphur thrown into it, and a glass jar suddenly put over it, the combustion of the sulphur continues for a considerable time; dense bluish-white fumes fill the jar, and at last conceal the flame completely. The smoke soon subsides when the combustion is over, and the water rises slowly in the jar. By this process, the air in the jar loses uniformly 8 per cent. of oxygen; it retains the smell of sulphurous acid, even though allowed to remain over water for a week. But the smell disappears in an instant, if the air be passed through water. A portion of the water over which the jar stood, being treated with muriate of barytes, yielded a precipitate which weighed 8. An equal portion of the same water evaporated to one-fourth, yielded a precipitate which weighed 7.

Action of acids  
on the sulphites.

7. The action of the more powerful acids upon the sulphites deserves attention, because it serves to illustrate the nature of sulphurous acid. This action has been described with considerable minuteness by Fourcroy and Vauquelin; but as the result of my experiments differs a little from theirs, a few observations may not be unacceptable to the chemical reader. To prevent tediousness, I shall confine my remarks to sulphite of potash.

Action of sul-  
phuric acid upon  
sulphite of pot-  
ash.

When sulphite of potash is thrown into concentrated sulphuric acid, a considerable heat is evolved, a violent effervescence takes place, and the salt loses 48 per cent. of its weight.

weight. The heat of boiling water renews the effervescence, and occasions a loss of weight, amounting to 2 per cent. more. So that sulphite of potash, when treated with sulphuric acid, loses uniformly the half of its weight. Yet it contains only 43.5 per cent. of sulphurous acid. The additional 6.5 parts may be ascribed perhaps to the escape of sulphuric acid along with the gas; for it can scarcely be doubted that there is an affinity between them. When the sulphuric acid solution is set aside, brilliant plates of super-sulphate of potash soon make their appearance in it.

When sulphite of potash is thrown into muriatic acid, a violent effervescence ensues, but no increase of temperature; and the salt loses 31 per cent. of its weight. The heat of boiling water renews the effervescence, and occasions a farther loss of 16 per cent. making the whole loss amount to 50, as in sulphuric acid. From this experiment we see that muriatic acid does not expel the whole of sulphurous acid, unless assisted by heat: and in that case, a portion of the muriatic acid is driven off at the same time with the sulphurous. When the muriatic acid solution is set aside, beautiful arborescent crystals of muriate of potash make their appearance in it.

I dissolved 500 parts of sulphite of potash in water, and putting the solution in a Woulfe's bottle, caused a current of oxy muriatic acid gas to pass through it; the gas passed afterwards through a second bottle of water connected to the first by a bent glass tube. After the process the bottles were set aside, till the green colour, occasioned by the oxy-muriatic acid, disappeared, and the fetid animal odour which usually succeeds that colour, was become perceptible. From the first bottle I obtained, by means of muriate of barytes, a precipitate which weighed 777 parts, indicating the presence of 37.3 per cent. of sulphuric acid. But as the sulphurous acid originally present amounted to 43.5 per cent. had it been wholly converted into sulphuric acid, not less than 48.5 per cent. of sulphuric acid would have been obtained, there was a loss then of 11.2 per cent. of course, 10.5 parts of sulphurous acid must have been dissipated by the action of the oxy-muriatic acid. Accordingly the liquid in the second phial gave an abundant precipitate with muriate of barytes: and this precipitate, contrary to what I expected, consisted chiefly of

fulphite of barytes; for the greater part of it was soluble in sulphurous acid.

Nitric acid and  
sulphite of pot-  
ash.

When fulphite of potash is thrown into concentrated nitric acid, a violent effervescence takes place, and much heat is evolved, the loss of weight is 44.5; the liquid treated with nitrate of barytes, gives a precipitate, which indicates the presence of 39.6 of sulphuric acid. Hence we see that the loss of weight during the effervescence was owing chiefly to the escape of nitrous gas.

When the acid is diluted, the effervescence is violent, but no heat is evolved, and the smell of sulphurous acid gas making its escape is very perceptible. The loss of weight is only 12 per cent. the residuum, treated with nitrate of barytes, gave a precipitate indicating the presence of 43.2 per cent. of sulphuric acid. Here we see that most of the loss of weight was owing to the escape of sulphurous acid: yet the greater part was converted into sulphuric acid.

Composition of  
sulphite of pot-  
ash.

8. During the course of these experiments I had occasion to examine the composition of sulphate of potash; and as my results differ a little from those stated by others, it will be proper to notice some of them in this place.

When fulphite of potash is heated to redness in a platinum crucible, the residuum possesses the properties of sulphate of potash. It may be fused without any loss of weight, and when dissolved and crystallized again, we obtain the same salt as at first: 100 parts of this salt precipitated by muriate of barytes, yields a precipitate which, after being heated to redness, weighs, at a medium, 96 parts, indicating about 23 per cent. of sulphuric acid. Hence this sulphate is composed of

23 acid  
67 potash

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100

When sulphuric acid is supersaturated by means of carbonate of potash, we obtain by evaporation the common sulphate of potash of chemists: the same salt separates in crystals during the purification of the potash of commerce. When the salt is reddened in a platinum crucible, it loses 1.4 per cent. of its weight, and no more, though it be kept in fusion. Dissolved in water, and treated with muriate of barytes, it yields a precipitate which weighs 128.5; the mean of three experiments differing

differing from each other not more than 3.5 per cent.\* Hence it contains 30.84 sulphuric acid. This sulphate, then, is composed of

30.84 acid
67.76 potash
1.40 water
<hr/>
100.00

When sulphite of potash is left for some months exposed to the air, and then heated to redness, it yields with muriate of barytes a precipitate indicating about 38 per cent. of sulphuric acid.

The superfulphate of potash loses 26 per cent. in a red heat, and the remaining 74 parts dissolved in water, and treated with muriate of barytes, yield a precipitate indicating the presence of 30.4 sulphuric acid, whereas 100 parts of the superfulphate dissolved in water, without being previously heated, yield, with muriate of barytes, a precipitate indicating the presence of 38.4 sulphuric acid. Hence it follows that the salt is composed of

38.4 acid
43.6 alkali
18.0 water
<hr/>
100.0 ;

or, abstracting the water, of

46.4 acid
53.6 alkali
<hr/>
100.0

### III. Oxide of Sulphur.

Sulphur usually occurs in one or other of three states ; External characters of sulphur. namely, 1. A whitish powder, formerly distinguished by the name of *luc sulphuris*. 2. In rolls or flowers of a greenish yellow colour. This is the sulphur of commerce. 3. In the state of a reddish yellow, pitchy substance. This is commonly employed for forming the casts of medals, &c. known by the name of sulphurs.

\* Mr. Vauquelin affirms, in his Dissertation on the Potash of Commerce, that this salt yields with muriate of barytes a precipitate amounting to 22°, the weight of the salt. My experiment differs very much from this statement.

1. It

*Lac sulphuris*, or white sulphur, is precipitated from any liquid, is always of a white colour, sulphur and water; yellow sulphur is the pure. which gradually changes to greenish yellow when the sulphur is exposed to the open air. If this white powder, or *lac sulphuris*, as it is called, be exposed to a low heat in a retort, it soon acquires the colour of common sulphur; and, at the same time, a quantity of water is deposited in the beak of the retort. On the other hand, when a little water is dropt into melted sulphur, the portion in contact with the water immediately assumes the white colour of *lac sulphuris*. If common sulphur be sublimed into a vessel filled with the vapour of water, we obtain *lac sulphuris* of the usual whiteness, instead of the usual flowers of sulphur. These facts prove that *lac sulphuris* is a compound of sulphur and water. Hence we may conclude that greenish yellow is the natural colour of sulphur. Whiteness indicates the presence of water.

Sulphur rendered viscid and dark-coloured by fusion.

2. It has been long known, that when a considerable quantity of sulphur is kept melted for some time in an open vessel, it becomes viscid, changes its colour to a dark violet, and acquires a kind of pitchy appearance. The nature of this change has not hitherto been examined by chemists. Fourcroy, indeed, affirms, that the sulphur, in this case, is in the state of an oxide. But the assertion does not seem to have been the result of any positive experiment.

Does not succeed in a shallow vessel.

I have never been able to produce this change in the appearance of sulphur by heating it in a flat dish, where nothing impedes the volatilization, though I have kept it melted in a glass capsule on sand, heated to  $250^{\circ}$ , for ten hours together. But the change takes place in a short time, when a considerable quantity of sulphur is kept melted in a crucible; and the greater the quantity employed, the sooner the change is produced, and the more complete it is.

This supposed oxide is of a violet colour; is soft if poured into water; one-sixth denser than sulphur; and tough.

When sulphur, thus converted into a supposed oxide, is newly prepared, its colour is a dark violet, with the metallic lustre; not very unlike newly-melted muriate of silver, when seen by reflected light. If it be thrown suddenly, while in fusion, into water, it continues soft for a considerable time; and, as it hardens, the colour changes from purple to reddish yellow. When broken, it exhibits a fibrous fracture, composed of small prismatic crystals. Its specific gravity was 2.325. It was very tough, resisting, with a good deal of ob-

stimacy, the action of the pestle. The powder had a straw yellow colour. Its properties differ, we see, from roll sulphur, which is remarkably brittle, and whose specific gravity does not exceed 2.

To ascertain whether this supposed oxide really contained oxygen, I treated 100 parts of it with nitric acid till the whole was converted into sulphuric acid. The process was as tedious as the acidification of common sulphur, by means of the same acid. By nitrate of barytes I observed a precipitate, which, after being reddened in a platinum crucible, weighed 667, indicating 160 parts of sulphuric acid; the supposed oxide had absorbed, of course, 60 parts of oxygen. Hence we have sulphuric acid composed of

62.5 supposed oxide

37.5 oxygen

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100.0

But 100 part of pure sulphur would have absorbed nearly 64 of oxygen, and formed 164 of sulphuric acid. Hence it follows that the supposed oxide is composed of

97.6 sulphur

2.4 oxygen

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100.0

Though the result of a similar experiment was nearly the same; yet the proportion of oxygen is certainly too small to authorize us, in the present state of chemical analysis, to conclude that the supposed oxide really contains  $2\frac{1}{2}$  per cent. of oxygen: for so small a deviation from the composition of sulphuric acid, by acidifying common sulphur, as  $2\frac{1}{2}$  per cent. may, very probably, be owing to an error of analysis. At the same time the uniformity of my results inclines me to believe that this supposed oxide of the French chemists really contains some oxygen.

3. As no satisfactory result was likely to be obtained by exposing sulphur to heat and air, it became necessary to try the effects of those chemical agents which are capable of communicating oxygen to other bodies. Sulphuric acid could not be used, because sulphur converts it into sulphurous acid; the effect of nitric acid was well known; but the action of oxymuriatic acid had not been tried. Some of the foreign chemists, indeed,

It seems to contain a small portion of oxygen, though perhaps not enough to entitle it to the name of oxide.

Oxygenation of sulphur by compound agents.

indeed, affirm that sulphur takes fire when plunged into that gas; but they must have, some how or other, deceived themselves.

Oxygenation by oxymuriatic acid. Sulphur exposed to ox. mur. acid gas was converted into the new product of a fine red liquid.

I connected three Woulfe's bottles in the same manner, by means of glass tubes; furnished each with Welter's tubes of safety. The first contained an ounce troy of pure dry flowers of sulphur; the second was filled two-thirds with distilled water; and the third with a weak solution of crystallized carbonate of potash. A current of oxymuriatic acid gas was made to pass through these bottles in the usual way. The process lasted a considerable time. The first bottle was soon filled with the greenish fumes of the gas; the sulphur gradually became moist and doughy, and the particles of it which adhered to the sides began to trickle down in drops; its colour changed to orange, and at last, a fine red liquid made its appearance. The whole of the sulphur was gradually converted into this liquid. I then stopt the process. Abundance of gas had passed through all the bottles: the water in the second was, at one time, quite milky, but it recovered its transparency before the process was finished. Dots of sulphur were deposited along the glass tubes which connected the first and second phials; but none in that which connected the second and third, the solution in the third phial effervesced precisely as in the usual process for preparing hyper-oxymuriate of potash. The gas which escaped was carbonic acid. No oxymuriatic acid gas could be distinguished by its smell in the small capsule of water into which a tube issuing from the third bottle was plunged.

— denominated sulphureted muriatic acid.

As the red liquid obtained by this process has never before been examined by chemists, and, as it differs very much in its properties from all other substances at present known, it will be necessary to distinguish it by a peculiar name, I shall call it *sulphureted muriatic acid*, till some better name be thought of.

— more than double the weight of the sulphur.

This liquid amounted to  $1\frac{1}{2}$  ounce measures, exclusive of what adhered to the sides of the bottle; its specific gravity was 1.623. It amounted, therefore, to 2.63 ounces, or more than twice the weight of the sulphur, exclusive of what had been volatilized during the process.

It is green by transmitted light.

Sulphureted muriatic acid is perfectly liquid; its colour is a fine red, intermediate between scarlet and crimson. When streaks of it run down the inside of the phial, they appear green by transmitted light.

When

When exposed to air, it smokes at first almost as intensely as the smoking oxy muriate of tin of Lebavius; but the intensity gradually diminishes, and, at last, resembles that of the most concentrated muriatic acid a little heated. It is very volatile, disappearing very rapidly when exposed to a moderate heat.

Its smell has a strong resemblance to that of sea-plants, but is much stronger. The eyes, when exposed to its fumes, are soon filled with tears, and acquire the same painful feeling as when exposed to the smoke of wood or peat.

Its taste is strongly acid, hot, and bitter, affecting the throat with a painful tickling.

It converts vegetable blue papers to red; but the change takes place slowly, unless the paper be dipped into water; the paper is not corroded unless heat be applied.

When brought near a phial of ammonia, dense white fumes of muriate of ammonia make their appearance. If it be held above a solution of nitrate of silver, yellow flakes precipitate in abundance,

If a drop of sulphureted muriatic acid be let fall into a glass of water, the surface of the water becomes immediately covered with a film of sulphur; a greenish red globule falls to the bottom, which remains for some time like a drop of oil, but at last is converted into yellow flakes; these flakes have an acid taste, which they do not lose, though allowed to remain in water for several days; they are very ductile, and continue so, though left exposed to the air.

4. To ascertain the constituents of this liquid I agitated 110 parts of it in a very weak solution of potash, and then threw the whole on a filter: a yellow ductile substance was separated, which adhered very strongly to the filter; what I could separate was put on a plate of glass, and dried gently in the open air. It weighed 40. This substance had a yellow colour, and resembled half dry oil paint; its taste was hot; it adhered so obstinately to the finger, that several days elapsed before every trace of it disappeared. When digested for some time in hot water, it fell into flakes of sulphur, and the water acquired an acid taste. The flakes weighed only 34, and possessed all the properties of common sulphur. The water in which it had been digested yielded, with nitrate of barytes, a precipitate which weighed 8, indicating the presence of 1.92 sulphuric acid with nitrate of silver; the precipitate amounted to 16, indicat-

It smokes, and is very volatile; taste acid; red- blue colour; gives white fumes by approach of ammonia; and when dropped into water, leaves a film of sulphur, and gives yellow acid, ductile, insoluble flakes that continue soft in the air.

Chemical examination of the red liquid or sulphureted muriatic acid.



Chemical examination of the red liquid or sulphureted muriatic acid. ing 2.88 muriatic acid : but the precipitate had a brownish tinge, of which muriate of silver is destitute. The 40 parts of yellow residuum then contained

34.00	fulphur
1.92	fulphuric acid
2.88	muriatic acid
1.20	water or loss
<hr/>	
40.00	

Seventy parts still remain to be ascertained. They must exist in the solution of potash. This solution, superaturated with nitric acid, and treated first with nitrate of barytes, and next with nitrate of silver, yielded precipitates indicating the presence of 4.8 fulphuric acid and 36.45 of muriatic acid. The residuum yield no farther precipitate with silver ; but, when evaporated to dryness, some yellow crystals were obtained, which gave traces of fulphur, but in too small quantity to be weighed. This analysis gives us the following proportions :

35.00	fulphur
39.33	muriatic acid
6.72	fulphuric acid
<hr/>	
81.05	
28.95	loss
<hr/>	
110.00	
<hr/>	
31.82	fulphur
35.75	muriatic acid
6.10	fulphuric acid
<hr/>	
73.67	
26.33	loss

or per cent.

This enormous loss was owing, at least, in part, to the improbability of separating from the filter, the whole sulphureous mass so as to weigh it. This induced me to have recourse to the following method of ascertaining the proportions of fulphur in sulphureted muriatic acid.

Remarks on the sulphuret of muriatic acid.

When sulphureted muriatic acid is thrown into warm nitric acid a very violent effervescence takes place, and the whole mixture is thrown, with a kind of explosion, out of the vessel. If the acid be cold, the effervescence is at first slow, but heat is very soon evolved, and the same effects produced. When the proportion of nitric acid is great, and the sulphuret dropt in

in very slowly, the effervescence continues moderate; nitrous gas and oxymuriatic gas being evolved, as was evident, from the smell. I dissolved 100 parts of the sulphuret in nitric acid. The liquid yielded, with nitrate of barytes, a solution which, when properly dried, weighed 282 parts, indicating 67.6 parts of sulphuric acid; but 67.6 of sulphuric acid contain 26.3 oxygen. Hence 100 parts of sulphureted muriatic acid contain 41.3 of sulphur.

Chemical examination of the red liquid or sulphureted muriatic acid.

If a crystal of muriate of barytes be suspended in sulphureted muriatic acid, no precipitate takes place; neither was any obtained by agitating this salt in powder with the sulphuret. Hence I conclude, that the sulphuret does not contain sulphuric acid; but that sulphuric acid is formed whenever the liquid comes in contact with water. The oxygen cannot be supposed to have been previously united to the muriatic acid solution. For I find, by trial, that if the oxymuriatic acid gas be still made to pass through the sulphuret, after it is once formed, sulphuric acid immediately makes its appearance in it. The oxygen then must have been combined with the sulphur; the whole of which was in the state of an oxide. Whenever the sulphuret is diluted with water, that oxide undergoes decomposition, one portion of it abstracting the whole of the oxygen from the other, so that sulphuric acid and sulphur make their appearance together.

If we suppose the proportion of sulphur and sulphuric acid obtained by the first analysis to be that which is formed when the sulphur is mixed with water, we shall have the oxide of sulphur composed of

	31.82 sulphur
	6.10 sulphuric acid
or of	35.54 sulphur
	2.38 oxygen
or per cent. of	93. 8 sulphur
	6. 2 oxygen
	<hr/>
	100. 0

But the sulphuret yielded to nitric acid 41.3 per cent. of sulphur. Hence it must contain 44 per cent. of oxide of sulphur. Sulphureted muriatic acid then contains

44.00 oxide of sulphur
35.75 muriatic acid
20.25 loss
<hr/>
100.00

Chemical examination of the red liquid or sulphureted muriatic acid.

The loss is still very considerable. Probably the greater part of it is owing to the presence of water; the amount of which it is impossible to ascertain.

The above analysis conveys but an inadequate idea of the constitution of sulphureted muriatic acid, because the proportions of its constituents vary considerably, according to the process. The longer the process is continued the greater is the increase of muriatic acid, and the smaller the proportion of oxide of sulphur. I found a portion of sulphuret thus formed to contain

47.1	muriatic acid
35.2	oxide of sulphur
4.0	fulphuric acid
<hr/>	
86.3	
13.7	loss

It was this last sulphuret that was obtained in the process in which the quantity of sulphuret which I got, as stated above, was measured, the second Woulfe's bottle contained a solution of muriatic acid and fulphuric acid in water. Hence we see that the sulphuret, after being formed, had been partly covered over by the oxymuriatic acid gas. The fulphuric acid obtained, by means of barytes, amounted to 36 grains; the muriatic acid to 139 grains. The third vial contained no fulphuric acid, but consisted of a mixture of muriate of potash, hyperoxymuriate of potash and carbonate of potash.

Remarks on the sulphureted muriatic acid.

5. The sulphuret of muriatic acid claims the peculiar attention of chemists, not only on account of its composition, which our previous knowledge would have induced us to consider as impossible, but on account of the many remarkable properties which it displays. As I mean to reserve a full account of its properties for a subsequent paper, I shall satisfy myself at present with the following remarks.

Sulphuret of muriatic acid dissolves phosphorus.

1. Sulphuret of muriatic acid dissolves phosphorus cold with great facility. No effervescence takes place; the solution has a fine amber colour, and is permanent. When evaporated, the phosphorus remains behind with a little sulphur, and at last takes fire. When the solution is mixed with liquid potash, the whole becomes beautifully luminous, and phosphuret of sulphur is precipitated.

Sulphuret effervesces with alcohol, and forms ether.

2. When mixed with alcohol, a violent effervescence is produced, ether is immediately disengaged, and, what I did not

not expect : this ether is mixed with sulphurous acid, and must be rectified in the same way as sulphuric ether, which it resembles in smell.

3. All the acids decompose this sulphuret, sulphur usually is decomposed precipitating, except liquid sulphurous acid, which produces by all acids ; no change, and nitrous acid, which dissolves and decomposes it at the same time.

4. The fixed alkalies dry produce with it a violent efferves- habitudes with cence and a very high degree of heat. When ammoniacal <sup>alkalis ;</sup> gas is passed through it, the vessel is filled with a fine purple sal-ammoniacal smoke, the whole becomes solid and of a deep red colour ; but when mixed with water, sulphur is immediately precipitated.

5. This liquid precipitates silver of a yellow colour mixed precipitates silver with white, the white is a muriate of silver, the yellow is a <sup>ver yellow and white.</sup> compound of the oxides of silver and sulphur. It becomes brown when dry. Nitric acid decomposes it, dissolving the silver and acidifying the sulphur.

## IX.

*Further Experiments and Observations on the Efflorescences of Walls. In a Letter from Dr. BOSTOCK.*

To Mr. NICHOLSON.

SIR,

SINCE I sent you the analysis of the saline efflorescence Four efflorescences from different walls. from the walls of Mr. Earle's house, inserted in your journal for November last, I have had an opportunity of examining four other efflorescences obtained from brick walls, the particulars of which I shall now detail.

The first of these was presented to me by my friend Dr. The first proved Rutter, who discovered it in considerable quantity on the top <sup>to be pure sulphate of mag-</sup> of the walls of his house just below the roof. This salt had <sup>as it is,</sup> in every respect the same external characters with the one which I had before examined, and upon submitting it to the action of the same chemical re-agents, similar results were obtained. In addition to the former experiments, I compared the effects produced upon it by pure ammoniac, and by the carbonate of ammoniac ; the former threw down a copious precipi-

precipitate, while the latter had no perceptible operation; a decisive test of the existence of magnesia, which was suggested to me by Mr. William Henry of Manchester. This salt appeared, therefore, to be a very pure sulphate of magnesia.

The second efflorescence was sulphate of soda with indication of muriatic acid.

The second efflorescence which I examined was obtained from the outer wall of a stable, which had been erected for some years. It differed on its appearance from the two former; instead of shooting out from the wall in spiculæ of considerable length, it appeared like a powder scattered over the surface, occupying distinct, round patches, so as in some degree to resemble the growth of a grey lichen. The bricks on which this efflorescence appeared were in general of a softer texture than those of the rest of the wall. Though it occupied a considerable extent, it was difficult to collect it in any quantity, but I obtained sufficient to subject it to the following experiments. The salt was dissolved in warm water, filtered and crystallized; the crystals were very soluble at the common temperature of the atmosphere. Muriate of Barytes added to the solution produced a copious precipitate; carbonate of pot-ash, pure pot-ash, pure ammoniac and oxalic acid were respectively added to the solution, but produced no effect. Nitrate of silver caused a precipitate, but only in small quantity. From these experiments it appeared that the salt in question consisted of the sulphuric acid, mixed with a small proportion of the muriatic, and combined with one of the fixed alkalies. From the form of its crystals I conceived that the sulphuric acid was in this case united to soda, but the quantity of salt which had been procured, was not sufficient to enable me to determine accurately from this circumstance. A more decisive test between the sulphate of soda and the sulphate of pot-ash, is the property which the latter alone possesses of forming alum with the acid sulphate of alumine. I accordingly prepared a quantity of this substance; to one portion of it sulphate of pot-ash was added, and to the other some of the salt under examination. By gentle evaporation and subsequent cooling, the first produced very evident crystals of alum, the latter only formed a confused mass. This I considered as a sufficient proof that this saline efflorescence was the sulphate of soda.

Third efflorescence sulphate

The third efflorescence was obtained from the inner walls of a brick house which was then erecting; it had all the internal cha-

characters of the saline efflorescence from the stable, and when of soda nearly submitted to the same chemical re-agents, differed from it <sup>pure</sup> only in exhibiting slighter traces of the muriatic acid; this salt was therefore a sulphate of soda nearly in a state of purity.

The walls of the salt water baths in this town are covered with a stucco, which is in several places blistered and mouldering away. The parts of the plaister which are decaying, are covered with a copious efflorescence, which has the appearance of a fine white down. Some of this I collected and examined. It was dissolved in warm water and filtered; the solution was not capable of being regularly crystallized, but formed a white mass, easily soluble, possessing the acrid taste of a fixed alkali, and affecting the colour of test papers in the same manner. A brisk effervescence was excited by the addition of an acid, and from this circumstance and the effect of the different re-agents, I conceived it to be one of the fixed alkalies. In order to determine whether it was an uncombined alkali, and to which of these bodies it ought to be referred, I ascertained what quantity of the sulphuric acid was requisite to saturate a known weight of the salt, and afterwards, employing the same acid, compared it with the quantity which the same weight of alkali required. As the salt had not attracted any moisture from the atmosphere during a period of some weeks, I concluded it to be soda, and I accordingly found that the same quantity of sulphuric acid saturated equal weights of soda, and of the salt under examination; the solution being slowly evaporated, formed well-marked crystals of the sulphate of soda. That part of the walls of the bath on which the salt had effloresced in the greatest quantity, was out of the reach of the immediate action of the sea water; but it is probable that the sand of the shore had been mixed with the lime, for by examining a quantity of water which had been digested upon a portion of the plaister, it yielded a very copious precipitation by the nitrate of silver, and this rendered it highly probably that the mortar contained the muriate of soda. It might therefore be conjectured that the soda in this case was accidentally formed by the same process which, according to Mr. Accum's account in the 2d vol. of the Journal, p. 243, is employed designedly in Prussia for obtaining it, by the decomposition of common salt.

I shall

**Investigation of the origin of the sulphate of magnesia on the bricks of Mr. Earle's house.**

I shall conclude this communication by detailing to you the progress which I have made in investigating the origin of the sulphate of magnesia, which appeared in Mr. Earle's house. Before proceeding farther, it will be proper to observe that the efflorescence was here altogether confined to the bricks, the mortar which united them being entirely free from it, and that rain water only had been employed in tempering the clay. It remained therefore to examine with accuracy, whether any salt, soluble in water, existed ready formed in the clay, and what were the component parts of the clay itself. To ascertain the first of these points, 60 grains of the clay powdered and dried were well washed with boiling water; the water was filtered and evaporated, and the residuum carefully collected; it did not weigh  $\frac{1}{4}$  of a grain. It was re-dissolved in water; it produced a copious precipitate with the muriate of barytes, and the nitrate of silver; a very faint cloud with oxalic acid and with pure pot-ash; ammoniac produced no effect. The ready formed salts appeared therefore to be the sulphate and muriate of a fixed alkali, with a minute portion of the muriate of lime, the whole however existing in very small quantity.

**The clay contained a portion of muriate and sulphate of alkali,**

**And a larger portion of magnesia.**

The clay itself was next examined; it was found to consist principally of silica and alumina in the proportion of about three to one; the quantity of lime was very small, though its existence was detected by the oxalic acid; its colour proved that it contained iron, and I also found that about five parts in the 100 consisted of magnesia. From this examination it appeared that one of the component parts of the salt exists in the clay; I attributed the formation of the sulphuric acid to the sulphur which is frequently met with in our coals, and which I conceived might unite with oxygen during the burning of the bricks. I attempted to put this action to the test of experiment, and accordingly I formed a paste of pipe clay and calcined magnesia in the proportion of 95 to five; this was placed in a crucible, surrounded with small coal mixed with a quantity of sulphur; the crucible was then kept for some time in a strong heat. I was not able to detect the presence of the sulphuric acid in the clay that was thus baked, but so many circumstances might actually take place in the formation and burning of the bricks, which we have it not in our power to imitate in the laboratory, that I do not consider the hypothesis disproved by my want of success.

**Experiment to shew whether sulphur from the coals had given the acid? Doubtful.**

In analysing the clay I proceeded nearly upon the plan <sup>Analysis of the</sup> pointed out by M. Vauquelin in the 30th vol. of the *Ann. clay, &c.* *de Chimie*.

This operation is however so tedious, and requires so much nicety in the management, that I made some attempts to ascertain the existence of magnesia in the clay by a shorter process. The first method which I employed was suggested by an observation of Mr. Kirwan; he states that alumine is sufficiently discriminated from magnesia by the greater solubility of the latter in dilute sulphuric acid; but it appears that the difference of solubility of these two substances cannot be employed as a test of the presence of magnesia where it exists only in small proportion; for I found that the sulphuric acid diluted with above 200 times its weight of water, after being in contact with pure alumine for the space of 10 minutes only, had acted upon the alumine so far that a precipitate was formed in the fluid by the addition of ammoniac.

The acetous acid is stated as possessing a much more powerful action over magnesia than over alumine; but upon trial the same objection occurred against its use as in the former instance.

The property which the magnesian salts possess of being decomposed by pure ammoniac but not by the carbonate of ammoniac, seemed to offer a method by which the sulphates of magnesia and of alumine might be separated when mixed together in solution, and by which means consequently the presence of magnesia might be detected in the clay under examination. But I found that though the sulphate of magnesia alone is not decomposed by the carbonate of ammoniac, yet that when a mixed solution of alumina and the sulphate of magnesia is subjected to the action of the carbonate of ammoniac, both the alumine and the magnesia are precipitated, so that when the fluid is separated by filtration, the addition of pure ammoniac produces no farther effect:

After I had made the unsuccessful experiment related above respecting the formation of the sulphuric acid, I received the last number of your Journal, containing a communication from Mr. Gregor, in which he gives an account of the production of the sulphate of magnesia from the ashes of pit-coal. He attributes the production of this salt to the decomposition of the schistus and pyrites which are commonly found in coal, <sup>Mr. Gregor's experiment not applicable to the case in question, as coal ashes were not used,</sup> and



and by heating a mixture of these substances he succeeded in forming the salt artificially. The success of his experiment seems to prove the truth of the theory, at least in the particular instance in which he observed the efflorescence: but in those cases where the salt evidently proceeds from the substance of the brick, and where the magnesia has been found to exist previously in the clay, the idea of Mr. Gregor appears less applicable, so far as regards the origin of the magnesia; but in both the processes the sulphuric acid is equally supposed to be derived from the pyrites. Mr. Gregor suggests that coal ashes might have been mixed with the clay of which these bricks were formed; but I find upon enquiry that this was not the case.

I am,

Liverpool, Aug. 30,  
1803.

Your obedient servant,  
JOHN BOSTOCK.

## X.

*Philosophical Observations on the Causes of the Imperfection of evaporating Furnaces, and on a New Method of constructing them, for the economical Combustion of every Description of Fuel. By C. CURAUDAU, corresponding Member of the Pharmaceutic Society of Paris\*.*

On the combustion of fuel and construction of furnaces.

**NOTWITHSTANDING** the attempts already made to introduce economy in the use of the combustibles necessary to the manufactures, we still use them with considerable waste. In all cases a much greater quantity of fuel is consumed than is needful to keep up the ebullition in evaporating furnaces, or to produce the requisite temperature in furnaces for other purposes. It may be easily conceived that this superfluous consumption must in large establishments be attended with great loss, and must eventually tend to produce a scarcity of fuel in the market. It therefore becomes us for both reasons to endeavour to prevent a scarcity, of which future generations might with justice accuse us of being the authors, unless we seriously occupy ourselves in search of the methods of burning wood with more economy. Many very remarkable improve-

\* From the *Annales de Chimie*, No. 138.

ments have indeed been made in the construction of furnaces <sup>On the combustion of fuel and construction of furnaces.</sup> within these few years, but they are only advances towards perfection, and are yet very far from being carried to the extent of which they are capable. This will no doubt be the case with the alterations I am about to propose; for these will enable us to make new observations, which most probably will lead to further alterations still more important.

*Concerning Evaporatory Furnaces.*

The physical impossibility of raising the temperature in evaporating furnaces as they are at present constructed, is one of the causes which has always appeared to me most strongly inimical to their improvement. For it must not be imagined that the intensity of the heat will be in proportion to the quantity of matter in ignition, or that caloric will not be more copiously produced by the same quantity of combustible under certain circumstances than others; as for example, when the temperature is already very high, the products of heat from a combustible so situated will be much more considerable than those from the same combustible burned in a furnace, the temperature of which is constantly depressed by the evaporation of the liquid in the boiler.

To prove that it is only by virtue of a temperature already elevated that we can obtain an advantageous combustion, I shall take Argand's lamp, which will afford a comparative instance on a small scale of the effect produced by the intensity of heat during combustion. When these lamps have their glass chimney, they afford a very brilliant light, and the oil will emit no smoke. But if the chimney be taken off, the oil will immediately burn duller, the light will be less intense, and the wick will give out smoke. This effect shews that it is the current of air in the chimney, and the heat it keeps up round the wick, which contributes to the effect of the combustion. What still adds weight to this opinion is, that the perfection to which this kind of lamp is brought depends principally on the form and proportions of its glass chimney.

This example must then naturally lead us to think that evaporating furnaces, as they are made at present, cannot advantageously promote combustion, since the bottom of the boiler, which is continually kept at the same degree of heat by the evaporation of the liquid in boiling, constantly prevents the

On the combustion of fuel and construction of furnaces.

rise of the temperature, whence it results that the heat which is insufficient to produce a complete combustion of the inflammable particles, will rather produce gazification than oxigation. This volatilization of the particles of combustible bodies which escape combustion, and which pass successively into the state of permanent gases, will also absorb a quantity of heat necessary to their gaseous constitution, which, together with the effects of the current of air, will tend to lower the interior temperature of the furnace, and to impede the process of combustion.

These remarks, which perfectly agree with all the phenomena of combustion, shew that the oxygen of the atmosphere does not act with much efficacy on combustible bodies, except when they are immersed in it at an high temperature, and that to apply an intense and uniform heat to an evaporating furnace without loss of the combustible, it should be produced in a fireplace, having a current of air, and so far distant from the boiler that the temperature may be raised gradually and at pleasure. By this means all the particles of the combustible matter will be in a state favourable to their oxigation; and the whole quantity of radiant heat produced by the reaction of the oxygen upon the combustible, will be disengaged and employed without loss.

That which under similar circumstances conduces still farther to encrease the action of the oxygen, is its continual renewal. For the higher the temperature of a furnace is raised the more easily the outer air will enter; and so likewise when the ignition is carried to a high degree, it becomes necessary and advantageous to check the current of air, not by closing the door or lower opening of the furnace, as is generally done, but rather by contracting or even closing the upper aperture of the chimney. By this means the heat becomes concentrated in the body of the furnace, and has no other passage than through the liquid in the boiler.

This remark on the method of checking the current of air by the top of the chimney, may also be applied to furnaces of fusion, and in cases where it is required to maintain the heat of a metal without exposing it to the oxygenating action of a current of air in a state of ignition.

*General Remarks on the Construction of Furnaces.*

That part of the fire-place which is to support the greatest heat, should be made of very refractory bricks. The best cement or mortar for bricks, in all cases where a bad conductor of heat is required, is a mixture of equal parts by measure of tan and clay. The tan prevents the cement from cracking, and produces an adhesiveness which, when dry, gives it a great degree of firmness.

Furnaces may also be constructed with this mortar, and on the same principles with those of evaporation, which I am about to describe.

Furnaces intended for strong heat, should be externally covered with a thick wall, constructed with the mortar of tan. By this means very little heat will be lost. All furnaces should be so constructed as to have the power of closing the upper aperture of the chimney at pleasure, in order to check the combustion, and concentrate the heat within the furnace, whenever this becomes necessary. When the temperature is very high, it is particularly necessary to regulate the issue of the current of air so as to prevent its too speedy circulation through the furnace, which, in certain cases, is prejudicial to the success of the operation.

By uniting all these conditions in furnaces, a certain saving of one fourth of the fuel will be made, and the combustion will be produced without any appearance of smoke. I insist more particularly on this remark, because it is clearly and physically proved that no combustible can be completely burned if smoke be produced.

*Description of an Evaporating Furnace, in which the Temperature may be raised at Pleasure.*

For common furnaces, the aperture of the vault A, Plate Description of a furnace for evaporation. VI. should be four decimetres ( $15\frac{1}{4}$  inches) wide, by three decimetres and a half ( $13\frac{1}{4}$  inches) in height; B is the part of the vault in which the combustion is performed. This vault must be at least two metres ( $6\frac{1}{2}$  feet) in length. C represents a boiler of one metre and a half (nearly five feet) in diameter, and of the same depth; it is set in a brick furnace.

The interval from the bottom of the boiler to the base of the furnace must be at most one decimetre (about four inches.) It must be observed in the construction of furnaces, that the brick-work should be gradually sloped towards the boiler, and to reduce

duce the space to about three centimetres (about an inch.) It must be thus continued to within one decimetre of the edge of the boiler; and must then be brought into contact with it. D is an aperture of two decimetres wide by one in height (about eight inches by four) communicating with E. But at the side of the angle *a*, this passage for the heat must be made of one metre (three feet three inches) in width, by one decimetre (about four inches) in height, and this proportion continued to the aperture E.

F is a second boiler, intended to be heated by means of the excess of heat from the first; many others may be applied in succession, if required. G is an aperture with the same proportions as D. At the angle *b*, it must be observed to make the aperture of the chimney five decimetres by two ( $19\frac{1}{2}$  inch. by  $7\frac{1}{2}$  inch.) and to continue this proportion to about two thirds of its height. The aperture may then be contracted so that, at its upper extremity, it may not be less than one decimetre by three (about four inches by twelve.) This part of the chimney should be so constructed as to be able to close it conveniently, when required.

## XI.

*Correction of a Mistake in Dr. Kirwan's Essay on the State of Vapour in the Atmosphere. By Mr. DALTON.*

To Mr. NICHOLSON.

S I R,

Mistake of Dr. Kirwan in quoting Mr. Crosthwaite as authority for a maximum height of the clouds.

I TAKE the liberty of requesting you to correct a mistake in Dr. Kirwan's essay on vapour, copied in your last number, page 246. In treating upon the height of clouds, he observes, "In lat.  $54^{\circ}$ , in Cumberland, Mr. Crosthwaite observed none " lower than 2700 feet, and none higher than 3150, in the " course of several years." For this he refers to my meteorological observations, page 41. The facts there stated, however, are so very different from those above mentioned, that I conclude Dr. Kirwan has not seen the book, and has been misinformed. The account referred to is, that Mr. Crosthwaite observed the heights of the clouds usually three times a day for five years, by remarking their intersection with Skiddaw (a high mountain near Keswick.) The result was,

Clouds from 0 to 100 yards above Derwent Lake, 10 times.

100 - 200	-	-	-	-	42
200 - 300	-	-	-	-	62
300 - 400	-	-	-	-	179
400 - 500	-	-	-	-	374
500 - 600	-	-	-	-	486
600 - 700	-	-	-	-	416
700 - 800	-	-	-	-	367
800 - 900	-	-	-	-	410
900 - 1000	-	-	-	-	518
1000 - 1050	-	-	-	-	419
above 1050	-	-	-	-	2098

Total 5381

His observations could not be particular above 1050 yards, that being the perpendicular height of Skiddaw. There is not therefore any *maximum* of height so much as hinted at; and the *minimum* is 0, or when the clouds rest on the ground, an event occurring in every part of Great Britain two or three times a year. Were we to form a conjecture from the above observations relative to the greatest height at which clouds are formed in this country in ordinary, it would be about 1 mile; but in summer they are probably sometimes  $1\frac{1}{2}$  mile above the level of the sea.

Considering the great service that Dr. Kirwan has rendered to meteorology and chemistry, and my own obligations to him on those accounts, it is unpleasant for me to signify dissent from the doctrine he inculcates respecting the state of vapour in the atmosphere. At the same time that his interesting series of essays in the eighth volume of the Transactions of the Royal Irish Academy were in the press, my essays on the force of vapour from water and other liquids, both in a vacuum and in air, and on evaporation, published in the Manchester Transactions, Vol. V. Part 2, were also in the press. He holds the notion of a chemical solution of water in air; and I maintain that vapour subsists in air as it does in *vacuo*, constituting a peculiar atmosphere, mixing but not combining with any of the gases of the compound atmosphere. On my principle the density of the aqueous atmosphere at any height is totally independent of the density of the compound mass of air, and is to be ascertained by knowing the density of vapour at the earth's

No maximum was given or aimed at by that author's observations.

Observations, or short statement of Dr. Kirwan's and Mr. Dalton's theories of the state of atmospheric vapour.

earth's surface, and its specific gravity; in the same way as we would ascertain the density of the oxygenous or azotic atmospheres, or one of hydrogen, at any given height, having the like *data*.

It has been a matter of surprise to me, that most or all of my essays published in the volume above mentioned, have been copied and circulated in one or other of our periodical publications, except those two just mentioned, which appear to me by far the most important, and which seem too to have been considered as such by the foreign journalists.

I am your's, &c.

J. DALTON.

Manchester, Aug. 22, 1803.

## XII.

*Cheap and effectual Method of securing Beams of Timber in Houses or elsewhere, which have been injured by the Dry Rot, or are decayed by Time.* By MR. JAMES WOART.\*

Easy method of securing decayed timbers in buildings, &c.

WHERE the ends of the girder are decayed by time, or injured by the dry rot, they are often taken out, and new ones put in their place, at a great expence: and if the dry rot is in the walls, the ends of the new girder will be in danger of it again: such was the case at Eltham, in Kent, where in one house there were three new girders to one floor in the space of twenty years; whereas my method will be found infallible, executed at much less expence, and not subject to the dry rot, because the end of the girder may be cut off clear from the wall; and if an air grate is put on the outside, so as to admit air to the end of the girder, it will remain safe from injury.

Plate IX. Fig. 2.†—A, shews the end of the decayed girder, with the braces applied upon it.

B B,

\* Memoirs of the Society of Arts, 1802. A reward of twenty guineas was awarded to the inventor, who, in an introductory letter states, that by the iron braces, of less cost than 20l. he secured the house of Hannege Legg, Esq. at Putney in Surry, which could not have been done by new beams without loss, derangement, and charges to the amount of eight hundred pounds.

† There are two plates in the Transactions, the second of which  
for me

**B B**, the templets or wall-plates on which the girder rests. Easy method of securing decayed timbers in buildings, &c.  
**C C C C**, one of the iron levers for raising and supporting the girder (there being a similar one on the opposite side.)

This lever is moveable on a pin **D**, which comes through a hole in the lever, distant about two feet from the end of the girder. This pin forms part of a collar **E** bedded in the girder. The lever is six feet long, three inches wide, and three fourths of an inch thick, and extends from the wall-plate along the side of the girder.

The extremity of the lever is moveable on another pin **F**, projecting through it from an upright iron **G**, bedded in the side of the girder, and carrying a nut and screw, which act on a cross plate **H**, through which the upright iron passes.

At the other end of the lever, next the templet, is an iron collar **I**, bedded in the girder, which collar may be raised or lowered at pleasure, by means of the nut and screw **K**, forming part of it; and by aid of the cap-plate **L**, which presses upon the lever, and also clasps it to the girder by its bend at **L**.

As *Plate IX. Fig. 2*, shows only one side of the girder, and, as has been before observed, there being also a similar lever on the opposite side of the girder, their separate parts, method of connecting them, and their mode of action, are more fully explained in *Plate VII. Fig. 1, 2, 3*, where the same letters are made use of to point out the several parts.

*Fig. 1.*—**E**, shows the whole of the collar to be bedded in the side and bottom of the girder, and the pins **D D**, on which the two levers are moveable.

*Fig. 2.*—The cap-plate **H**, the two upright irons **G G**, with their nuts and screws, which act upon the extremities of the two levers by means of their pins **F F**.

*Fig. 3.*—The collar **I**, on which that end of the girder next the templet rests, the sides of which collar are bedded in the girder. **C C** are the claws or bended legs of the two levers which go into the templet. **L** is the cap-plate, **K K** are the nuts and screws.

At Mr. Legg's house, where the levers above mentioned were applied, the beams of the roof were so decayed that the roof was in imminent danger, the bearings were entirely rot-

forms *Plate VII.* of our present number. The other unfortunately was neglected to be sent to the engraver's, and the mistake not discovered till too late. It will be given in our next.



Easy method of securing decayed timbers in buildings, &c.

ten, and the beams were sunk three fourths of an inch, and pressing against the wall for support; if there had not been a large cornice underneath, supported by brackets, the whole roof must have fallen.

To put them in order, I first put shores or supports under each end of the two beams, on which the double roof lay, and then forced the four shores at once, for the security of the roof, the work, and men. The iron levers, C, were then prepared, let into the templet, and fixed on each side of the beam, on the pins D, projecting from the collar E, bedded in the beam, about two feet from its end. When the whole apparatus was ready, on screwing the nuts on the upright irons G, at the extremity of the levers, the beam was raised to its proper height with great ease, although it was supposed there was above two tons weight on each beam, on account of the lead gutter, and gutter-beam betwixt the double roof, and the rich ornamented ceiling attached to the joist, which was not in the least destroyed except where the iron collar E was fixed, which was put up from the under side by cutting the ceiling the width of the collar. These beams were so decayed, and so hollow, that the common method of bolting plank on each side of the beam would not have been safe; and if it could have been executed, the new planks would have been subject to the dry rot, and the roof still in danger, which is now prevented, as the iron is not affected by it. The beam-ends were cut clear from the walls, and the beams are suspended by means of the iron levers, whose feet rest on the templets of the walls. An air grate was made, on the outside of the wall, to admit a current of fresh air to the ends of the timbers. The roof is now much safer than when originally made, as the timber is secured from decay; and, owing to the collar E, the bearings are now two feet shorter at each end of the beam; the bearing on each beam being now, in the whole, four feet shorter than in its original state.

After the beams were brought to their proper height, and the levers and screws adjusted, screw-bolts were put into the timber, through holes purposely left in the lever, betwixt D and F, and the whole work thus perfectly secured.

At the other end of the girder, M, *Plate IX.* is shown another method of supporting timbers, where the ends are decayed.

The

The particular irons used in this way are shown in Plate VII. *Easy method of securing decayed timbers in buildings, &c.*  
 Fig. 4. N is a collar for the girder; O, an iron frame which rests on the templet; P P, two nuts which raise the collar N. R R show the clawed ends of the two bars of iron, extending under the girder, bedded therein, and screwed to it at their extremities, about five feet distant from the templet.

Fig. 5, is one of the iron bars last mentioned.

S is the claw or lap which projects over the collar N. T is the place where it is screwed into the girder.

Fig. 6 and 7. Plate VII. explain a third method of securing decayed timbers.

Fig. 6, gives a side view of a decayed girder: *a*, represents the templet; *b*, an iron lever, six feet long, nearly straight, being only cambered one inch, three inches wide, and three quarters of an inch thick; this lever extends along the side of the girder *c*, and is secured firmly to it by the side irons *d d d d*, which are two inches wide, and full half an inch thick, pointed at the ends. The higher ends of these side irons are driven into the girder, and the lower points pass through holes in the lever into the lower part of the girder, and are held close to the girder by staples *eeee*: the side iron next the templet may be fixed slanting, in order that it may enter sounder wood. A claw, *f*, which is part of the lever, rests on the wall plate *a*, and is bedded in it; an iron plate, *g*, lying under the girder, and let into it, passes through the lever at *h*, connecting it with a similar lever on the opposite side, and which assists in the same way to support the girder: *i* is a flooring joist, to show how deep the levers are inserted therein.

Fig. 7, shows the under part of the same girder; *b b*, are the bottoms of the two levers above mentioned, fixed to the girder by the side irons and staples before described; *k k*, the broad feet of the levers which lie flat upon the wall plate; *ff*, the two claws projecting from the feet, in order to bed in the wall plate; *iii* are joists, partly cut through, to admit the iron levers to lie close to the girder: *g* shows the iron plate or collar on which the girder bears; it is turned up an inch and a half at each end, to keep the levers close to the sides of the girder. This collar should be made out of inch-bar iron, with points projecting from it, in the same manner as the collar at D D, Fig. 1, to connect it with the levers, by passing through holes made through them for that purpose.

To fix the levers, put a shore two feet six inches from the wall, under the girder, to support it; then cut off the decayed end, and take out the templet, or part of the wall plate, if decayed; and put in a stone templet for the irons to rest upon, with mortices in the stone to admit the claws of the lever: then fit the collar underneath the girder, two feet from the wall, to answer the holes in the lever; make an incision in the joists three-fourths of an inch wide, and three inches deep, to admit the levers; fix the levers on each side with the collar, so as to force up the levers together; then with slight shores force up the ends of both levers together, and fix the side-irons firm. The girder will thus be perfectly safe.

The templet or wall plates, on which the levers rest, are made of Portland stone, three feet long, nine inches wide, and five inches deep, with incisions or mortices made therein for the claws of the levers.

Certificates, confirming Mr. Woart's improvements, were received from the commissioners of the navy, from Mr. Joseph Harris, smith, at Putney, and Mr. George Smith, surveyor, at Putney.

### XIII.

*Account of the Method of extreme Branch Grafting. By the Inventor WILLIAM FAIRMAN, Esq.\**

SIR,

**Introductory  
observations on  
the useless trees  
in orchards.**

FROM much conversation with Mr. Bucknall on the idea of improving standard fruit-trees, we could not but remark that in apple orchards, even in such as are most valuable, some were to be seen that were stunted and barren, which not only occasioned a loss in the production, but made a break in the rows, and spoiled the beauty and uniformity of the plantation.

To bring these trees into an equal state of bearing, size, and appearance, in a short time, is an object of the greatest importance in the system of orcharding, and also for the

\* In a letter to Charles Taylor, Esq. Secretary to the Society of Arts, and inserted in their Memoirs for 1802. For which the Silver medal was awarded.

recovery

recovery of old barren trees, which are fallen into decay, not so much from age, as from the sorts of their fruits being of the worn-out and deemed nearly lost varieties.

Having long entertained these thoughts, and been by no means inattentive to the accomplishment of the design, I attempted to change their fruits by a new mode of engrafting, and am bold enough to assert that I have most fortunately succeeded in my experiments; working, if I am to be allowed to say it, from the errors of other practitioners, as also from those of my own habits. New method of grafting.

My name having several times appeared in the Transactions of the Society for the Encouragement of Arts, &c. and having the honour of being a member of that Society, I thought no pains or expence would be too much for the completion of so desirable an improvement. Under these impressions, and having many trees of this description, I made an experiment on three of them in March 1798, each being nearly a hundred years old. They were not decayed in their bodies, and but little in their branches. Two of these were golden pippins, and the other was a golden rennet. Each likewise had been past a bearing state for several years. I also followed up the practice on many more the succeeding spring, and that of the last year, to the number of forty at least, in my different plantations\*.

The attempt has gone so far beyond my most sanguine expectation, that I beg of you, Sir, to introduce the system to the Society, for their approbation; and I hope it will deserve the honour of a place in their valuable Transactions.

I directed the process to be conducted as follows: Cut out all the spray wood, and make the tree a perfect skeleton, leaving all the healthy limbs; then clean the branches, and cut the top of each branch off where it would measure in circumference from the size of a shilling to about that of a crown piece. Some of the branches must of course be taken off where it is a little larger, and some smaller, to preserve the canopy or head of the tree; and it will be necessary to take out the branches which cross others, and observe the arms are left to fork off, so that no considerable opening is to be perceived when you stand under the tree, but that they may represent an Instructions for preparing the trees.

\* The average expence I calculated at 2s. 6d. each tree.

uniform head. I must here remark to the practitioner, when he is preparing the tree as I directed, that he should leave the branches sufficiently long to allow of two or three inches to be taken off by the saw, that all the splintered parts may be removed.

Position of the grafts.

The trees being thus prepared, put in one or two grafts at the extremity of each branch ; and from this circumstance I wish to have the method called *extreme branch grafting*.

Cement and general preparation and management.

A cement, hereafter described, must be used instead of clay, and the grafts tied with bafs or soft strings. As there was a considerable quantity of moss on the bodies and branches of the trees, I ordered my gardener to scrape it off, which is effectually done when they are in a wet state by a stubbed birch broom. I then ordered him to brush them over with coarse oil, which invigorated the growth of the tree, acted as a manure to the bark, and made it expand very evidently ; the old cracks were soon, by this operation, rendered invisible.

All wounds should be perfectly cleaned out, and the medication applied as described in the Orchardist, p. 14. By the beginning of July the bandages were cut, and the shoots from the grafts shortened, to prevent them from blowing out. I must here, too, observe, that all the shoots or suckers from the tree must enjoy the full liberty of growth, till the succeeding spring, when the greater part must be taken out, and few but the grafts suffered to remain, except on a branch where the grafts have not taken : in that case, leave one or more of the suckers, which will take a graft the second year, and make good the deficiency. This was the whole of the process \*.

Great advantages of leaving the tree of its full size.

By observing what is here stated, it will appear that the tree remains nearly as large when the operation is finished, as it was before the business was undertaken ; and this is a most essential circumstance, as no part of the former vegetation is lost, which is in health fit to continue for forming the new tree.

—and increasing its powers.

It is worthy of notice, that when the vivifying rays of the sun have caused the sap to flow, these grafts inducing the fluid through the pores to every part of the tree, will occasion in-

\* The system succeeds equally well on pear, as also on cherry trees, provided the medication is used to prevent the cherry tree from gumming.

numerable

numerable suckers or scions to start through the bark, which, together with the grafts, give such energy to vegetation, that in the course of the summer the tree will be actually covered over by a thick foliage, which enforces and quickens the due circulation of sap. These, when combined, fully compel the roots to work for the general benefit of the tree.

In these experiments I judged it proper to make choice of grafts from the sorts of fruits which were the most luxuriant in their growth, or any new variety, as described in the seventeenth and eighteenth volumes of the Society's Transactions, by which means a greater vigour was excited; and if this observation is attended to, the practitioner will clearly perceive, from the first year's growth, that the grafts would soon starve the suckers which shoot forth below them, if they were suffered to remain\*. With a view to accomplish this grand object of improvement, I gave much attention, as I have before observed, to the general practice of invigorating old trees; and I happily discovered the error of the common mode of engrafting but a short distance from the trunk or body, as in Fig. 1. Pl. VIII. There the circumference of the wounds is as large as to require several grafts which cannot firmly unite and clasp over the stumps, and consequently these wounds lay a foundation for after-decay. If that were not the case, yet it so reduces the size of the tree, that it could not recover its former state in many years, and it is dubious if it ever would; whereas, by the method of extreme grafting, as Fig. 3, the tree will be larger, in three or four years, than before the operation was performed. For all the large branches remaining, the tree has nothing to make but fruit bearing wood; and from the beautiful verdure it soon acquires, and the symmetry of the tree, no argument is necessary to enforce the practice.

Fig. 2 was my first experiment about eight years since. The error of No. 1 was there a little amended, and gave me the idea of engrafting at the extremity. Permit me to remark, that those done in my orchards, on the plan of Fig. 2, did not, neither were they able to bear so many apples last season, which was a bearing year, as those on the plan of Fig. 3,

\* This thought should be kept in suspense, as ten years hence it may appear otherwise. However, they will be valuable trees, and highly profitable, as will any other brought under the same system.

which.

which produced me about two bushels each tree of the finest fruit I had in my orchards, from the third summer's wood only. Some engrafted with Ribston pippins were beautiful.

Approbation of  
Mr. Bucknall.

Mr. Bucknall visited me this summer for the express purpose of seeing my trees; and he says the manner of conducting the system is the happiest that ever was conceived. For when a tree has done its best, and has continued to extreme oldage, just disposed to fall into dissolution, as also when this is the case with trees in a stagnated and barren state, they are thus renovated, and may, with the greatest probability, continue valuable for fifty years to come. I need not say, do not make the attempt when the energy of growth is over; that will easily be seen by the body and arms, but more particularly from the size, figure, shape, and colour of the leaves, which give the proper indication of health or decay in vegetation.

Should the Society desire it, several gentlemen resident here, will gladly send up certificates to confirm the statements.

I remain, SIR,

Your most obedient servant,

W. FAIRMAN.

*Millers-House near Sittingbourn, Kent,*

*Feb. 9, 1802.*

#### CEMENT FOR ENGRAFTING.

Cement for  
engrafting.

One pound of pitch

One do. . . . rosin

Half do. . . . beefwax

Qtr. do. . . . hogslard

Qtr. do. . . . turpentine

To be boiled up together, but not to be used till you can bear your finger in it.

S I R,

Testimonial of  
S. D. Bucknall,  
Esq.

THIS is to certify to the Society for the Encouragement of Arts, &c. that William Fairman, of Millers-House, Lynsted, Esq. has long been a steady and zealous promoter of the improvement of the standard fruits of the country; and that he planted one entire orchard, of sixteen acres, ten years ago.

The system of extreme-branch grafting, now introduced to the public; he has had in contemplation full eight years, though not in its present style of success and elegance; for he has been improving. In those operated upon within the last  
three

three or four years he has been wonderfully successful, and I am happy in an opportunity of adding my testimony to the advantages resulting from this method of renovating old fruit-trees.

An idea equal to the present system could not have fallen into better hands than those of Mr. Fairman. He is blessed with a good soil, cultivates the land well, and steadily attends to improvement. The gentlemen of the committee, by looking at the three little sketches of drawings which represent the three trees, will see that *Fig. 1* is so amputated, as not likely to continue in health, so as again to form a good tree; and that *Fig. 2* will be many years before, if ever it does. But there are now many fine large trees in the state of *Fig. 3*, which have been engrafted but three or four years, and yet, as far as structure goes, are complete already, and in two years much fine fruit may be expected.

The system is as follows: Make the trees perfectly clean, and keep them as uniformly large as is convenient.

In autumn, 1801, I spent some days at Lynsted, and several times walked over the plantations with Mr. Fairman, and was very much pleased with their appearance.

I remain, Sir,

Your obedient Servant,

THOMAS SKIP DYOT BUCKNALL.

February 22, 1802.

*Reference to the Engraving of Mr. FAIRMAN's Method of Extreme-Branch Grafting; Plate VIII. Fig. 1, 2, 3, 4.*

*Reference to the engravings.*

*Figure 1.* displays the old practice, commonly called cleft-grafting.

*Fig. 2.* Improved experiment on *Fig. 1*, by engrafting higher up the tree.

*Fig. 3.* Shows the method of extreme-branch grafting, recommended from experience, by Mr. Fairman. Two grafts or scions are there placed at the extremity of each branch; besides which, additional grafts are inserted in the sides of the branches; as, at AAAAAA, or where they are wanted to form the tree into a handsome shape.

*Fig. 4.* Shows upon a larger scale than the former figures the method of applying the grafts at the extremity of the branches, and retaining them by the bass-mat bandage and cement.



## XIV.

*Observations on several Pharmaceutical Preparations, by CIT. STEINACHER, Druggist at Paris. Abridged by CITIZEN PARMENTIER\*.*

*Unguentum Nutritum.*

Unguentum nutritum.

CITIZEN Dubree, an eminent druggist at Rouen, has lately presented a formula for unguentum nutritum, to the Pharmaceutical Society. As apothecaries zealous for the perfection of their art, have proposed improvements in the preparation of this ointment at different periods, I have thought that an object to which the attention of practitioners has been called from time to time, notwithstanding it is apparently obsolete, deserved a fresh examination †.

The mixture of oil, vinegar, and litharge. Requires carbonic acid, to convert the litharge into carbonate, and thicken the oil.

Too much vinegar will prevent the combination.

The formula of the French pharmacopœia the best.

When oil, vinegar, and litharge are to be mixed together into a homogeneous mass, a little litharge must be dissolved in acetous acid ‡, and a sufficient quantity of carbonic acid must be introduced, 1st. to convert the greater part of the litharge into white carbonate, which remains diffused through the oil; 2dly, to thicken the oleous mixture, an effect analogous to the thickening of soups by the carbonic acid, with which we were made acquainted by Pelletier. If a sufficient quantity of vinegar to form a saturated saline compound be employed, the mixture will never combine perfectly. This theory, founded on experiment, brings us back to the prescribed formula, as the best that can be adopted, that which produces an ointment, the most bulky, the lightest, and the most cooling to the part

\* *Annales de Chimie*, XLVII. 97. (No. 139.)

† Dr. Aikin, the learned editor of Lewis's *Materia Medica*, says; "The *unguentum nutritum*, made without heat, though now expunged from our dispensatories, is much the best of the ointments prepared from lead, and a very excellent application in many cases. It should not be long kept, but made fresh as wanted." H.

Litharge completely soluble in acetous acid, but not in common vinegar. The residuum of the latter.

‡ Experience has taught me, that levigated litharge is completely soluble in a sufficient quantity of acetous acid, but that the final residuum of its solution in common vinegar, which has been supposed to be superoxidized lead, contains only tartrate and malat of lead, with a great deal of extractive matter, which form a paste with a remnant of the litharge reduced toward the metallic state.

affected.

affected. It succeeds very well, when the operator is endued with patience, and works in a cold place. It may be abridged however, if, according to the excellent advice of Baumé, we employ coagulated oil of olives, by which the surfaces of contact are increased, and the introduction of the air is facilitated. One important fact with respect to keeping the preparation is, that at the temperature of  $15^{\circ}$  or  $16^{\circ}$ , at which most kinds of fermentation take place, a portion of the carbonic acid is extricated, and leaves exposed an oxide at  $\gamma_{\Delta\gamma}$ , which becomes again yellow. It requires a temperature of  $10^{\circ}$  to preserve its white colour unaltered.

The process may be shortened by employing coagulated oil.

Warmth spoils it.

But it will keep well in the cold.

Citizen Dubree, and Citizen Granet before him, proposed to expedite the preparation by adding hog's lard; but I find, that this addition diminishes its bulk and levity. In Germany different compositions are made under the name of *nutritum*, as with vinegar of litharge and half its weight of oil of roses, which produce an ointment as white as wax, and of the consistence of a liniment; with vinegar of litharge two parts, and olive oil three parts, which yield a whitish ointment of a moderate consistence; with two parts of olive oil, one part of wax, and two parts of vinegar of litharge, which furnish an ointment of a firm consistence, and a waxy whiteness. But all these compositions are simple mixtures, feebly united, by no means resembling the nutritum of the French shops, and not requiring for their formation a mutual reaction between the different particles of the ingredients.

The addition of hog's lard injurious.

Different compounds called nutritum in Germany.

All these simply mixtures.

### *Crystallization of Phosphoric Acid.*

It is known, that the affinity of phosphoric acid for water overpowers its force of crystallization; in fact this salient substance appears commonly in the form of a thick oil. I have lately observed, however, that time, the grand producer of regular crystallizations, effects a symmetrical combination between its particles.

Phosphoric acid commonly a thick fluid; yet crystallizable by time.

I had prepared half a kilogramme of phosphoric acid, according to the method of Lavoisier, with phosphorus and nitric acid, both of them extremely pure. This acid, freed from nitrous gas, reduced to the consistence of a thick syrup, and introduced into a phial with a glass stopper, had been used at different times in the course of a year, without exhibiting any peculiar appearance. The year following I let it remain per-

fectly in it by repose. Crystals formed in it by repose.

These crystals are prismatic in thin shining laminæ.

fectly at rest in the phial, which was half full, and closely stopped. After this period I found the surface of the fluid covered with a saline crust, from which shot downward prismatic crystals in shining laminæ, an inch long, and a line broad, diverging from a centre. I will not describe their geometrical structure, for they are extremely thin, and embedded in a fluid too viscous for me to take them out without breaking. Besides, they are still increasing; laminæ rise from the bottom of the vessel, which touch the surface of the glass, and seem preparing to intermix with the ramifications that shoot down from the upper stratum. The sides of the vessel are the seat of this beautiful crystallization. The centre remains in part concrete or fluid, whence it follows, that if a very regular dissipation of the particles of the liquid acid of phosphorus be occasioned by repose, the sides of the vessel contribute to it in great measure by affording fixed points, to which the positions of affinity most favourable to crystallization direct themselves.

#### *Purity of Phosphorus.*

Charcoal combines with phosphorus during the distillation.

Proust has informed the public, that, in the distillation of phosphorus, a combination of this substance with the charcoal constantly took place. This important discovery extends much farther than its celebrated author has shewn.

Phosphorus purified in Woulfe's method.

Take the most brilliant and most transparent phosphorus, which has not only been strained through chamois leather, according to Woulfe's method, but has also been dissolved several times in nitro-muriatic acid, as done by Count Muffin-Puschkin, or which has been treated with oxygenated muriatic acid, after the mode of Mr. Juch of Wurzburg; let it be heated gently in a long slender tube; red parts will separate from it. Put a few grains of this phosphorus, which is conceived to be so pure, on a silver spoon, and set fire to it; a red trace will remain. If the spoon be heated in the dark, the red trace will be seen still to burn, and a coal will remain impregnated with phosphoric acid.

Muffin Puschkin's,

or Juch's, still shews marks of it.

Heating with caustic alkali incapable of proving the purity of phosphorus.

Mr. Juch has asserted, that his phosphorus is extremely pure, because it no longer becomes black when heated with caustic alkali; but it is in fact because the phosphure of carbon is unalterable by caustic potash. According to the indisputable authority of Proust, this re-agent is incapable of proving the purity of phosphorus. I confess, that heated oxygenated

Heated, oxygenated muriatic

muriatic

muriatic acid destroys part of the carbone of phosphorus, be- acid destroys  
 cause the combustible power of its oxigen increases in the part of the car-  
 ratio of its elasticity; but it produces this effect only by burn- bone.  
 ing a proportionate quantity of phosphorus. On the contrary, Cold separates  
 when it is cold, and its oxigen is reduced to its natural degree it in the state of  
 of elasticity, it is far from destroying the carbone, it separates black oxide.  
 it in the state of black oxide, and converts the phosphorus into  
 white oxide, while at the same time, itself returns to the state  
 of simple muriatic acid. I have observed this fact on a stick  
 of transparent phosphorus, which I kept two years in a bottle  
 filled with pure oxygenated muriatic acid, saturated at the tem-  
 perature of 10°. It is impossible, therefore, to free the phos- Impossible to  
 phorus entirely of charcoal. They oxide, or are acidified nearly free phosphorus  
 in proportional quantities; and though the proportion of char- from charcoal  
 coal may be diminished, the phosphorus always retains some by completely.  
 its power as a whole. In fine, I am obliged to contradict the  
 assertion of an illustrious master, Citizen Fourcroy, "that we Mistake of  
 are unacquainted with any direct combination between car- Fourcroy,  
 bone and phosphorus, though it probably exists," and to con-  
 sider that product on which chemists have hitherto bestowed  
 the name of pure phosphorus, as a kind of gangue, from which Pure phosphorus  
 the radical phosphorus is disengaged to enter into a number of yet unknown  
 combinations, without our being capable of obtaining it in its  
 primitive form.

### *White Oxide of Phosphorus.*

When phosphorus is heated in a very long and very slender glass tube, in a sand-heat of 100° of the decimal ther- Mode of con-  
 mometer, it is covered with a mild light, and exhales a white verting phos-  
 vapour, which condenses in the upper part of the tube, while, phorus into a  
 at the same time, part of the phosphorus, with excels of white oxide at a  
 carbone, separates with its red colour. This white vapour, minimum.  
 which has acquired for its formation a slight combustion, is a  
 white oxide of phosphorus at a minimum. The following are  
 some of its properties. It is flocculent, possessed of cohesion, Some of its pro-  
 and occupies four times the space of the phosphorus employed perties.  
 in the experiment. When it is dry, it does not redden lit-  
 mus paper. It contains caloric, and inflames on the contact  
 of combustible substances. It powerfully attracts the moisture  
 of the air, and is rapidly converted into phosphorus acid. It White oxide of  
 differs greatly from the white oxide of phosphorus made, by phosphorus at a  
 the maximum.

**Its properties.** the long action of water, or cold oxygenated muriatic acid, This appears friable and pulverulent. It has lost almost all its latent heat. It is very little inflammable, and does not attract the moisture of the air. It is acidifiable only by the intimate action of an oxygen that contains caloric highly condensed, as that of the nitric acid. In a word, it is phosphorus at a maximum of oxidation.

### *Regular Crystallization of Essential Oil of Roses.*

**Regular crystallization of oil of roses.**

Citizen Steinacher has lately observed this with attention. He mixed eight kilogrammes of the magna of damask roses (*roses pâles*) with some parts of water, according to the process of Cit. Demachy; and after a day's maceration he drew off by distillation sixteen kilogrammes of water. This was immediately poured into a large glass jar, which was covered with a cloth, and left at rest. In twenty-four hours he found the surface of the water covered with an iridescent

**Resembles the crystals of snow,**

pellicle, interspersed with little hexahedrons, very much resembling the crystals of snow, which the illustrious Cit. Mongé has described. He informs us, that a slight shake is sufficient to tear the crystalline gauze, and reduce it to that irregular form of whitish scales or laminæ, which the oil of roses commonly assumes.

**and requires absolute repose.**

## SCIENTIFIC NEWS.

*Extract of a Letter from Dr. SCHAUB to Mr. PARKINSON, dated Cassel, July 2, 1803.*

**Tungsten not acidifiable.**

I AM busily employed in the analysis of various minerals, the results of which I shall communicate to you in my next. I have noticed among other things also, that the metal called tungsten (Wolfram by the Germans) can only be obtained at the highest degree of de-oxidation, and that this metal does not belong to the class of acidifiable metals; for tungsten cannot be oxidized by means of common processes of oxidation.

**New method of obtaining pure prussic acid.**

I have discovered a new method of obtaining prussic acid, in a state of absolute purity. This process consists in pouring upon one part of prussian blue, half a part of sulphuric acid, diluted

with an equal quantity of water, and subsequent distillation. The prussic acid passes over in the alcohol; its odour greatly resembles the water of the lauro cerasus.

It is a deadly poison to animals. Perhaps these notices may interest the London chemists, &c. &c.

*Annotation by the Translator.*

The following method of obtaining tungsten, I believe has not been made known in this country. It is recommended by Richter \* a German chemist.—F. A. Richter's method of obtaining tungsten.

Let equal parts of tungsten oxide (tungstic acid) and dried blood be exposed for some time to a red-heat in a crucible; pass the black powder which is formed into another smaller crucible, and expose it again to a violent heat in a forge, for at least an hour. Tungsten will then be found, according to this chemist, in its metallic state in the crucible.

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*Meteoric Stones.*

C. BIOT, member of the National Institute, in a letter to the French Minister of the Interior, dated July 20, 1803, gives a detailed account of his inquiries, &c. respecting a fire ball which exploded in the neighbourhood of Laigle. The memoir will be separately printed. Extraordinary meteor and shower of stones in France.

On Tuesday, April 26, 1802, about one in the afternoon, the weather being serene, there was observed from Caen, Pont-Audemer, and the environs of Alençon, Falaise, and Verneuil, a fiery globe of a very brilliant splendour, which moved in the atmosphere with great rapidity.

Some moments after there was heard at Laigle, and in the environs of that city in the extent of more than thirty leagues in every direction, a violent explosion, which lasted five or six minutes.

At first there were three or four reports like those of a cannon, followed by a kind of discharge which resembled a firing of musketry; after which there was heard a dreadful rumbling like the beating of a drum. The air was calm and the sky serene, except a few clouds, such as are frequently observed.

\* Richter ueber die neuen gegenstände der Chemie. Part I. p. 49.

The noise proceeded from a small cloud which had a rectangular form, the largest side being in a direction from east to west. It appeared motionless all the time that the phenomenon lasted. But the vapour of which it was composed was projected momentarily from the different sides by the effect of the successive explosions. This cloud was about half a league to the north-north-east of the town of Laigle; it was at a great elevation in the atmosphere, for the inhabitants of two hamlets a league distant from each other saw it at the same time above their heads. In the whole canton over which this cloud hovered, a hissing noise like that of a stone discharged from a sling was heard, and a multitude of mineral masses, exactly similar to those distinguished by the name of *meteoric stones*, were seen to fall at the same time.

The district in which the stones fell forms an elliptical extent of about two leagues and a half in length and nearly one in breadth, the greatest dimension being in a direction from south-east to north-west, forming a declination of about  $22^{\circ}$ . This direction which the meteor must have followed is exactly that of the magnetic meridian; which is a remarkable result.

The largest of these stones fell at the south-east extremity of the large axis of the ellipse; the middle-sized ones fell in the centre, and the smallest at the other extremity. It thereby appears that the largest fell first, as might naturally be supposed.

The largest of all those which fell weigh  $17\frac{1}{2}$  pounds. The smallest he saw weighed about two gros, which is the thousandth part of the former. The number that fell is certainly above two or three thousand. They were friable some days after their fall, and smelled strongly of sulphur. Their present hardness was acquired gradually.

*Abstract of a Memoir on the Febrifuge Principle of Cinchona,*  
by CIT. SEGUIN \*.

THE object proposed to himself by the author in the task he undertook was, to point out the means of knowing with certainty the true febrifuge principle of cinchona, to distinguish the species that contain it from those that do not, and finally to appreciate its quantity and quality.

Hitherto the sight and taste have been the only tests of the presumable qualities of the peruvian bark of the shops; but as these have no precise standard, and are inapplicable to powdered bark, they very imperfectly indicate the presence of the febrifuge principle. It was of importance, therefore, to substitute to these means, little better than illusory, others not only capable of calculation, but likewise invariable. Chemical re-agents alone can answer these ends.

In consequence Cit. Seguin began by isolating the respective properties of all medicinal substances, and he examined the action they exert on all other chemical substances.

These researches led him to develop very decisive characteristics in the febrifuge principle of cinchona; which place it in a perfectly distinct class. The following are its characters.

It precipitates the solution of tan, but not the solutions of gelatine and sulphate of iron.

When cinchona has not all these characters, it is a proof that it is mixed with something else, or that it does not contain the febrifuge principle.

The author has subjected to this analysis all the known species of cinchona, found among all the druggists and apothecaries of Paris and Versailles, and constantly obtained the same results.

Unfortunately these researches have shown, that but an infinitely small quantity of good, unmixed cinchona, is to be procured in the shops; the greater part being either destitute of the febrifuge principle, or mixed, or of a very inferior quality, though containing no mixture.

These results are of so much the greater importance, because the effects of different kinds of cinchona in fevers are only in proportion to the greater or less quantity of the febrifuge principle they contain; and those which contain none, as well as all the substances that may be mixed with them, are more or less injurious to the system.

The experiments of Cit. Seguin on the febrifuge principle of cinchona, having convinced him that most of the bark found in the shops was injurious or inefficacious, because it was spoiled by keeping, adulterated by mixture, or deprived of the febrifuge principle; he has endeavoured to obtain a febrifuge principle always the same, more efficacious, more certain in its effects, more capable of assimilation with our system, and so cheap, that there could be no temptation to adulterate it.

Sight and taste insufficient to judge of the goodness of bark.

The febrifuge principle of bark very distinct.

Its characters.

Various specimens of bark subjected to this test.

which proved that very little good is to be found in the shops.

Efficacy of cinchona proportionate to the quantity of febrifuge principle it contains.

Defects of the common bark of the shops.



To attain this important object, the author has inquired what the true cause of fevers, as of their effects, is; what the nature of the febrifuge principle of cinchona, and what its action on our system. He has subjected to the action of the re-agents pointed out for the febrifuge principle of cinchona, all chemical and medicinal substances; and assured himself, whether such of these substances, as might contain the febrifuge principle, did not contain, at the same time, other substances prejudicial to the animal economy. Lastly, he had to cure fevers by the help of these remedies, and then confirm this theory by repeated experiments. Such is the course Cit. Seguin has pursued.

The febrifuge principle is gelatine.

The new febrifuge principle, which he proposes to substitute instead of cinchona, because it unites all the advantages of the bark, without any of its inconveniences, is gelatine in its pure state.

Advantages this possesses over bark.

Considered in a medical, economical, and political view, gelatine promises much greater advantages than bark, in its application to the cure of fevers. It occasions no irritation; procures quiet sleep and gentle perspiration; keeps the belly open, without producing colic or nausea: has no unpleasant flavour; restores the strength, and is digested even by the weakest stomach, that would reject the bark as soon as administered.

Disadvantages of bark.

On the other hand, cinchona irritates the system, disturbs the sleep, has a disagreeable taste, frequently occasions costiveness, and is very indigestible.

Comparative cheapness of gelatine.

In an economical view, there is still greater difference between cinchona and gelatine; the price of the latter being to that of the former at most as one to thirty-two.

Lastly, gelatine is indigenous, cinchona is not; and the purchase of the latter requires us to send abroad a very considerable sum of money, which might be kept at home by adopting the use of gelatine.

Cases cured by it.

To this memoir the author has subjoined thirty-seven cases, in which he performed a cure with gelatine, under the eyes of some respectable physicians, and he has desired a Committee to be appointed, to repeat his experiments, and report upon them.

Committee appointed to examine its effects.

Accordingly Citizen Portal, Desessarts, Hallé, Fourcroy, Berthollet, and Deyeux, have been nominated for this purpose.

Their experiments are made at the School of Medicine, in a room exclusively appropriated to these inquiries; already a great number of patients have been cured; and the Committee will soon make their first report on these cases, <sup>which appear to be confirmed by experience.</sup>

*Query by a Correspondent respecting the Augustine Earth.*

To Mr. NICHOLSON,

SIR,

WE possess many excellent elementary works on Chemistry, both original and translations, such as Thompson, Accum, Murray, Henry, Parkinson, La Grange, Green, Fourcroy, Jacquin, &c. but in none of these authors is mentioned the method for obtaining the new earth, called *Augustine*; although most of these works have been published a considerable time after this earth was made known by the German discoverer, Professor Tromsdorf. I have also made enquiry, concerning this subject, of most of the public teachers of chemistry, and other individuals, who stand high as chemical philosophers, but in vain; I will therefore thank you to allow these lines a place in your valuable Journal. Perhaps one of your Correspondents will be kind enough to favour me with the process for obtaining this earth; for the author of these lines cannot find it in the mineral which is said to contain it; having pursued the usual methods of examining mineral substances for that purpose.

I am,

SIR,

Your's, &c.

P. O.

*Spaniard said to resist high Degrees of Heat and strong chemical Agents.*

THE prints of Paris, and some of our own, too implicitly Extraordinary copying them, have for some time exhibited a strange narrative of a young Spaniard, born at Tolosa, and now 23 years of age, of whom it has been very particularly affirmed, <sup>story of a Spaniard said to resist heat and corrosive acids.</sup>

That

That though his skin exhibited no appearance of peculiarity, either natural, or indicating preparation by art, yet without injury, 1. He bathed his feet for six minutes, and washed his hands and face in oil heated to  $250^{\circ}$  of Fahrenheit, which is 38 degrees hotter than boiling water. 2. He did the same with a solution of sea salt, heated 12 degrees higher. 3. He stood with his naked feet upon a bar of iron near the welding, or at the white heat; he held the bar in this state in his hands, and rubbed it on the surface of his tongue. 5. He washed his mouth with the strongest sulphuric and nitric acids, and applied the same to the other parts of his skin, with no other effect than that the nitric acid produced a yellow tinge; and 6. he remained a considerable time in an oven heated to within 18 degrees of the boiling water point.

Though our reasoning from analogy in matters of experiment, is liable to mislead, as well by infusing too much doubt as too much confidence, yet I should have passed over this tale, without notice, if I had not heard of it from very respectable correspondents. I suppose there may be something extraordinary in the degree of insensibility of the subject in question, as the Institute has paid attention to him; but I understand that the story is now told with great abatements. Citizen Pinel, a man of information, and well known as an accurate observer, is commissioned to report upon the same; and I have no doubt but his account will shew how much easier it is for men to tell falsehoods than to reverse the course of nature.

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*Method of giving Malt Spirits the Flavour of Brandy.\**

Flavour of malt  
spirits amended.

INTO a quart of malt spirits put three ounces and a half of finely powdered charcoal, and four ounces and a half of ground rice. Let these ingredients remain during fifteen days, only observing to stir them often: at the expiration of this time, let the liquor be strained, and it will be found to be much improved.

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*Preparation of a Lute proper for Chemical Operations. By  
C. PAYSSÉ, Professor of Chemistry.†*

Lute, used and  
checked, not baked  
time.

IN the preparation of the oxygenated muriatic acid in the large way, the necessity I found for a lute, which, to the ad-

\* From the *Bibliothèque Physique Economique*, No. 10. An. XI.  
† From the *Annales de Chimie*, No. 137. An. XI.

vantage

vantage of being cheap, should add these of being easily prepared; of resisting the action of the destructive vapour of the acid, as well as the strong heat which the luted part is often required to bear; which should be easy of application, and in an uniform manner, and not harden too quickly; obliged me to make some experiments on the subject, the result of which have been very satisfactory.

After making a great number of mixtures with different substances, I made choice of the following, which produced me a homogeneous composition, drying as slowly as could be desired, extremely hard; when dry, of a very compact texture; and, in short, possessing all the properties I had desired.

Take the white of two eggs, with their yolks, and of powdered carbonate of lime, or of quick lime well flaked in the air, about half the weight of the eggs; spread it on a cloth, and apply it as a lute.

#### NOTE.

THIS lute, the composition of which is very simple, possesses a degree of elasticity, when dry; I have formed vessels of it, which are impermeable to water, and susceptible of being polished on the wheel. This composition resembles the substance of which the pipes, called *Meerschaum*, are made.

#### *Two new Quadrupeds.\**

Two living Quadrupeds have lately been received at the Museum of Natural History at Paris, which are entirely unknown among naturalists, and were brought to Europe by Captain Baudin. Professor Geoffroy (of Egypt) who has inserted a description of them in the annals of the Museum, calls them *Fascolomes*. They come from the western coast of New Holland; their fur promises to be of some use; and, according to the opinion of Captain Hamelin and his suite, their flesh is very excellent food. They are particularly interesting to naturalists from the singularity of their organization. In the form of the head, the number, arrangement, and nature of the teeth, and the form of the fore feet with which

Two new Quadrupeds from New Holland,

\* Decade Philos. No. 51. An. XI.

they burrow in the earth, they resemble the marmot; but they differ from them, by the female having a pouch beneath the belly, and by the whole structure of the organs of generation, in which they are similar to the *surique* of Buffon. The form of the hinder foot is the same as in that animal with a pouch; the thumb being distinct from the other fingers, and without a nail: the tail is so short, that it remains hid below the hair, which is brown, bushy, and very long. The *Fascolomes* of the Menagerie are yet young, but are already larger than rabbits. Their temper is admirable; they may be handled, or removed, without shewing any symptoms of fear, anger, or uneasiness; their movements are heavy and clumsy; they live under ground, sleep during the day, and go in search of food at night. In general they possess but little energy or activity; they scratch themselves like the monkey, and they may be fed with bread, milk,\* roots, and every sort of herbage.

*Preservation of Iron from Rust.*

\* Defense of iron from rust.

CIT. Conté has adopted a method, which he finds effectual, for preventing the oxidation of iron and steel; or, in popular terms, to prevent iron and steel from rusting. It consists in mixing with fat oil varnish, at least half, or at most four-fifths of its quantity of highly rectified spirits of turpentine. This varnish must be lightly and evenly applied with a sponge; after which the article is left to dry in some situation not exposed to dust. He affirms, that articles thus varnished retain their metallic lustre, and do not contract any spots of rust. This varnish may also be applied to copper, of which it preserves the polish, and heightens the colour. It may be employed with particular advantage to preserve philosophical instruments from any change, in experiments where, by being placed in contact with water, they are subject to lose that polish and precision of form which constituted part of their value.

## ACCOUNT OF NEW BOOKS.

*Philosophical Transactions of the Royal Society of London, for the Year 1803. Part I.*

THE Contents of this Part are, 1. The Bakerian Lecture, Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By William Hyde Wollaston, M. D. F. R. S. 2. A Chemical Analysis of some Calamines. By James Smithson, Esq. F. R. S. 3. Experiments on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures. By Mr. William Henry. 4. Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Being the Substance of a Report made to the Right Honourable the Lords of the Committee of the Privy Council, appointed to take into Consideration the State of the Coin of this Kingdom, and the present Establishment and Constitution of His Majesty's Mint. By Charles Hatchett, Esq. F. R. S. 5. Observations on the Chemical Nature of the Humour of the Eye. By Richard Chenevix, Esq. F. R. S. and M. R. I. A. 6. An Account of some Stones said to have fallen on the Earth in France, and a Lump of Native Iron said to have fallen in India. By the Right Honourable Charles Greville, F. R. S. 7. Observations on the Structure of the Tongue, illustrated by Cases in which a portion of that Organ has been removed by Ligature. By Everard Home, Esq. F. R. S. 8. Observations on the Transit of Mercury over the Disk of the Sun; to which is added an Investigation of the Causes which often prevent the proper Action of Mirrors. By William Herschell, LL. D. F. R. S. &c. 9. An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables; and on their Operation in Tanning. By Humphry Davy, Esq. Professor of Chemistry in the Royal Institution. 10. Appendix to Mr. William Henry's Paper, on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures.\*

## APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

\* Our Readers will observe, that we have as usual, reprinted in our Journal most of these valuable Papers.

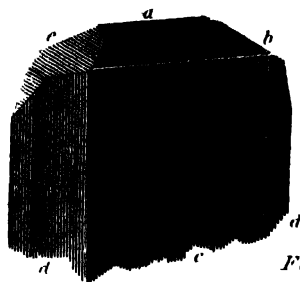
*An Essay on the Law of Patents for new Inventions ; to which are prefixed Two Chapters on the general History of Monopolies, and on their Introduction and Progress in England, to the Time of the Inter-regnum ; with an Appendix containing Copies of the Caveat, Petition, Oath, and other Formulæ, with an arranged Catalogue of all the Patents granted from the 1st of January 1800, to the present Time. By JOHN DYER COLLIER. 1803. Longman and Rees, Royal 8vo.*

One of the most obvious expedients for taxing the industry of man in social life, but at the same time one of the most pernicious, consists in monopolies. Accordingly we find in all governments that this resource is more or less adopted, and trades, manufactures, and various operations, become confined to the executive power, or what is worse, to the private favourites of men of influence. A long series of years have elapsed since this nuisance was abolished in our country, by the statute of James, and the monopolies that yet remain, are under the direct sanction of law, and so few, that a common observer would be disposed to say we have none.

A class of monopolies which has constituted the subject of a clause of exception in that act, consists in the sole working and making of new manufactures for a limited time under royal grant, to the first and true inventor thereof. It has been a subject of discussion whether even this exclusive privilege which is often made the instrument of public deception, and sometimes of oppression by wealthy Individuals to crush the industry of ingenious men by expensive legal processes under letters patent, for objects of public possession ; it has often been disputed whether this exclusive privilege be a benefit or an evil. The facts I think are, that many private fortunes are lost, in supporting pretended inventors, or in bringing real ones into effect, and that our arts, trade and sciences are greatly benefited by this last operation :—

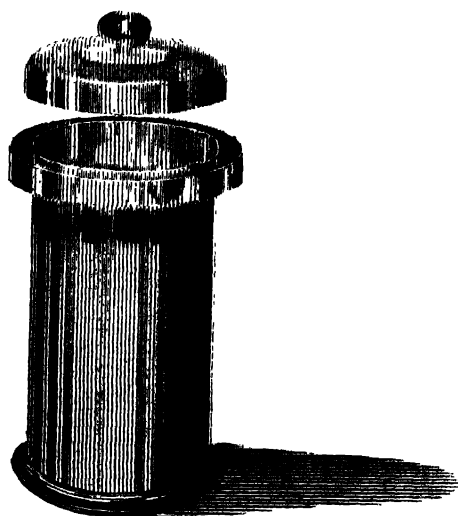
The subject of patents and monopolies in general is therefore of great interest and practical importance, and I have no doubt the public will receive this compendium as a valuable addition to their means of information respecting them.

*Electrical Galamine.*



*Fig. 1.*

*Fig. 2.*



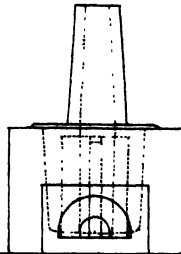
*Closure of Vessels  
for Anatomical Preparations &c.*



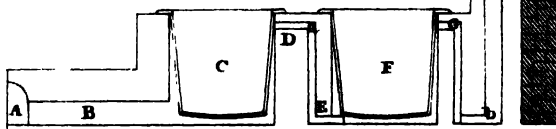


# *Furnace for Evaporation by regulated Temperature.*

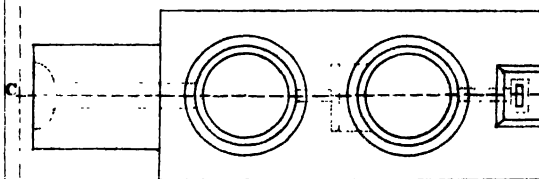
*Elevation parallel to the line A.B.*



*Section in the line C.D.*



*Plan.*



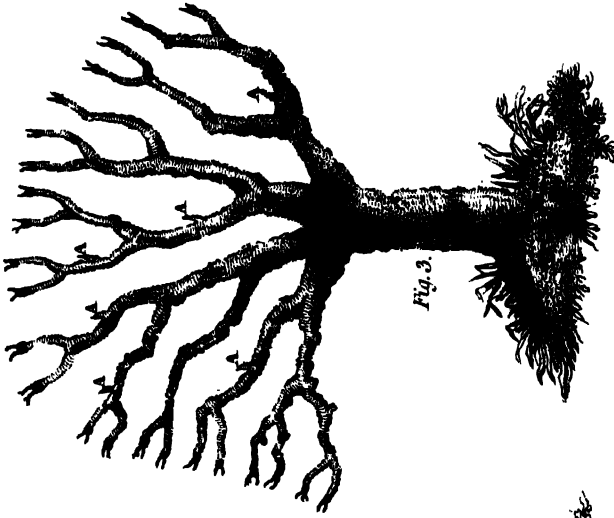
*Metres of 30 3 Inches*



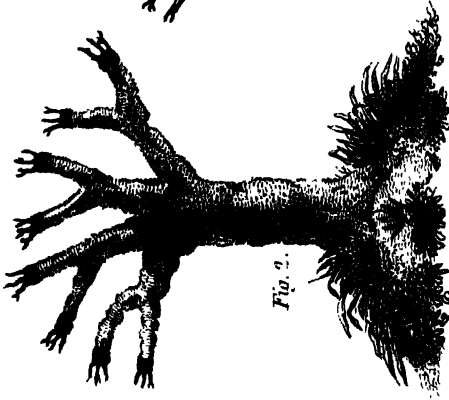




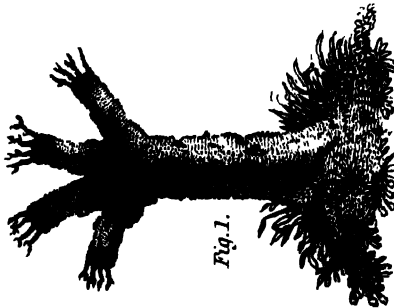
*W. & W. Fairman's. Method  
of Extreme Branch Grafting.*



*Fig. 3.*



*Fig. 2.*



*Fig. 1.*



*Fig. A.*



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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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NOVEMBER, 1803.

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ARTICLE I.

*Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Abstracted from the Memoir of CHARLES HATCHETT, Esq. F. R. S. in the Philos. Transf. for 1803.*

*(Concluded from Vol. V. Page 303 of our Journal.)*

*Experiment I.*

TWELVE pieces of standard gold were first examined, and were placed so that six were opposed to six. Loss of it near  
gold by friction.

The brass frame, in which each upper piece was fixed, weighed 1604 grains; and it was found necessary to add to each a weight of lead, equal to 19825 grains; so that the pieces were rubbed against each other under the pressure of  $19825 + 1604 = 21429$  grains = 3 lb. 8 oz. 12 dts. 21 grs \*.

\* This weight was not employed till repeated trials had proved the extreme difficulty, and almost impossibility, of producing any perceptible effect with less, in a moderate period of time; and, even with this weight, the experiments were found to be exceedingly tedious. The only evil which resulted from such a pressure was, that the comparative wear of the fine gold appeared much more considerable than would have been the case, if a small weight could have been employed; some observations will therefore be found in the subsequent pages, which point out the necessity of making an allowance for this circumstance.



The machine was then put in motion, until the index showed that 286690 revolutions had been performed; and, as a double crank acted during each revolution, the pieces were rubbed against each other alternately, in opposite directions, 573380 times, being twice the number of the revolutions.

The twelve pieces of standard gold, being taken out, were weighed, and were found to have lost 8,60 grs.

### *Experiment II.*

**Of gold alloyed with an unequal weight of copper.** Twelve pieces of gold combined with an equal proportion of copper, and without any impression, lost 103,11 grs. in 70340 revolutions.

### *Experiment III.*

**Of fine copper.** Twelve similar pieces of fine copper, lost 174,80 grs in 22200 revolutions.

Hence it appears, that standard gold loses less by friction than gold much debased by copper, and this less than copper alone.

**Gold alloyed with copper, silver, tin, and iron.**

The next series of experiments was made with gold differently alloyed; when gold 1. made standard by copper, 2. reduced to 18 carats by copper, 3. made standard by copper and silver, 4. made standard by silver, 5. of 23 car.  $3\frac{1}{2}$  grs. fine, 6. made standard by tin and copper, 7. made standard by iron and copper, 8. alloyed with an equal quantity of copper, was found to have lost in the following proportions, in the order in which they have been enumerated. 0 grs. 0 grs. 0,10 grs. 0,10 grs. 4,20 grs. 15,30 grs. 21,60 grs. 65,78 grs. The wear of the pieces alloyed with equal parts of gold and copper, and with iron and copper, was so rapid, that they were obliged to be taken out of the machine after 105480 revolutions; and those containing tin were worn so thin in 189000 revolutions, as to require being removed: the rest sustained 200300 revolutions, whence their comparative loss was still less than as above given.

**Gold similarly alloyed and stamped.**

This experiment being made on smooth, flat pieces, it was repeated with others of similar composition stamped with the die before described, only omitting the compound of equal parts of gold and copper, and adding pieces of standard silver and of fine copper. The number of revolutions were only 20680, and the pieces, taking them in the order already mentioned, now lost respectively: 0,60 grs. 4,80 grs. 1,20 grs.

3,50

3,50 grs. 4,60 grs. 13,80 grs. 7,60 grs. The standard silver Standard silver  
lost 3,70 grs. the fine copper 46,30. and fine copper.

From comparing the effects produced with the number of re- raised surfaces  
volutions, it is obvious, that much more is lost in the friction lose most.  
of embossed surfaces, than of plain.

The experiments were afterwards varied by placing pieces Friction be-  
of the different compositions in such a manner, that in some tween similar  
cases the friction should be between similar pieces, in others, and dissimilar  
between those differently alloyed. The results of all those pieces.  
experiments are tabulated, but they would occupy too much  
room to enter into them minutely; and though, from un-  
avoidable circumstances, some little inaccuracies occa-  
sionally occurred, they may be concluded, as Mr. H. observes  
to prove :

1st. That fine gold, or of 23 car.  $3\frac{1}{4}$  grs. when exposed to Fine gold.  
friction against gold of an equal quality, under the pressure of  
a considerable weight, suffers a very notable loss; and, al-  
though various circumstances seemed to indicate, that but  
little effect, in respect to abrasion, is produced under a less  
weight, yet it must be remembered, that the first case may  
occur.\*

Moreover, that fine gold, under all circumstances, is more  
subject to have any raised parts of its surface obliterated, than  
any variety of alloyed gold; not always, nor indeed so much,

\* It is proper to remark, that the preceding experiments were Wear of coin  
made under a much greater weight than can be supposed to operate in the inverse  
generally during the circulation of money; and as, by some pre- ratio of its du-  
vious experiments, a less weight was found to produce, during a cility.  
certain time, little or no effect, it may be suspected, that although,  
under a greater pressure, fine or very ductile gold sustains a greater  
loss than some of those which are reduced to standard, yet, under  
a less pressure, or such as that which most commonly prevails in the  
course of the usual wear of coin, the reverse may probably be the  
case; for then the same causes operate with less rapidity, during a  
long period of time. From many various circumstances, there is  
reason therefore to believe, that the wear of coin against coin of a  
similar quality is, under a small or very moderate weight, in the  
inverse ratio to the degree of ductility; but this is only to be un-  
derstood in the abovementioned case, of coin rubbed against coin of  
equal quality.

by actual abrasion, as by having the protuberant parts pressed and rubbed into the mass, in consequence of its extreme softness and ductility.\*

2d. That fine gold, or of 23 car.  $3\frac{1}{2}$  grs. when rubbed against the various kinds of alloyed gold, always or generally suffers the greatest comparative loss.

Standard gold.

3d. That gold reduced to 22 carats, or to standard, by silver, or by silver and copper, or merely by copper, suffers by friction, under general and similar circumstances, a smaller diminution than the fine gold abovementioned; and, with or without abrasion, the protuberant parts on the surfaces of these pieces remain much more permanent, under all circumstances, than those of the fine gold. The difference of wear between the three kinds of standard gold abovementioned, does not in reality appear to be very considerable; but, upon the whole, the preference may be given to gold alloyed with a mixture of silver and copper, or to that which has only copper for the alloy.

Gold with iron or tin.

4th. That gold made standard partly by the addition of iron or tin, sustains a greater loss by friction than either of the three kinds of standard gold above-mentioned.

18 carat gold, with copper.

5th. That gold reduced to 18 carats by copper, is more liable occasionally to wear, in a small degree, than the three kinds of standard gold which have been lately mentioned, provided that the friction takes place between pieces of equal quality; but, in the contrary case, the principal loss always falls on the soft or standard gold, when it is opposed to gold of 18 carats, which is considerably harder.

Gold much debased with copper.

6th. That gold more debased than that of 18 carats, such as gold alloyed with an equal proportion of copper, suffers very considerably more than any of the kinds hitherto mentioned, provided that the pieces are of the same quality; but, on the contrary, fine and standard gold experience a very great loss, when exposed to the action of this debased gold, while the loss of the latter is comparatively much less.

Disadvantage of softness in coin.

\* This is, however, of much consequence; for, although coin may not suffer by actual abrasion, yet, if the impression made upon it can so soon be destroyed, it follows of course, that the pieces become (although still allowed to be current) no better than mere blanks, or fragments of a bar or ingot.

7th. That the wear of standard silver appears to be nearly equal with that of fine gold; but more than that of gold made standard by silver or by copper, and less than that of gold much debased by copper.

8th. That, as gold which is not inferior to standard wears in general less than standard silver, so does this last suffer much less than copper,

The loss sustained by copper, when rubbed against copper, is infinitely more than that of the former metals; and, when these are exposed to the action of copper, they, as well as the copper, suffer a very considerable loss. This appears from the general results of these experiments, which prove, that pieces of metal which are the most subject to wear, are those which produce the greatest loss upon other pieces of metal, when rubbed against them; and it is remarkable, that *in such a case*, the loss does not always fall on one in preference to the other; so that the wear can only be considered in the aggregate, although one of the pieces may be regarded as the principal cause.

In order, however, to illustrate the results of the preceding experiments, as far as they concern the softer and harder kinds of standard gold, and to ascertain more fully the comparative wear of flat and smooth surfaces with that of such as were partly protuberant, an experiment was made, with two kinds of standard gold: 1st. Gold made standard by fine Swedish copper, which was very ductile; and, 2d. Gold made standard by a mixture of fine Swedish copper and dollar copper. This was as brittle as was compatible with rolling and stamping; and was prepared by melting gold made standard by fine Swedish copper, with an equal quantity of gold rendered brittle by the standard proportion of Swedish dollar copper, which was mentioned in the first section of this paper.

It may here be observed, that a distinction must be made between hard and brittle metal. If a metal is disposed to crack when rolled, without requiring any extraordinary force to enable it to pass the rollers, then it may be regarded as brittle; but, if it requires an extraordinary force to make it pass the rollers, and is not disposed to crack, then it may be considered as hard.

This

This experiment proves,

Very ductile  
and harder gold  
compared.

1st. That very ductile standard gold, when exposed to the friction of gold of a similar quality, suffers less by abrasion than gold which is comparatively brittle, or harder, and which is subjected to friction under the same circumstances.

2d. That when soft gold and brittle or hard gold rub against each other, the greatest loss is sustained by the soft gold. And,

3d. That pieces which have raised or embossed surfaces, suffer a greater loss, under every circumstance, than those which are smooth and flat.

Coin rubbing  
against coin, in  
ordinary circu-  
lation, loses but  
little.

The whole of the foregoing experiments were made with the machine called No. 1; and, as the friction was continued, in each experiment, during many days, with a pressure upon each couple of pieces equal to 3 lbs. 8 oz. 12 dts. and 21 grs., and as (considering the severity of such a trial) the loss sustained by the pieces, separately or collectively, was not very considerable, it may with reason be inferred, that standard gold does not easily suffer abrasion by the friction of metal against metal, or of coin against coin, especially under the circumstances which commonly prevail during the circulation of money.

In the machine No. 1, the pieces of gold were opposed face to face; it now therefore appeared proper, that the facts thus ascertained concerning the wear of gold, of different degrees of ductility, should be farther examined, and corroborated by a different method. To effect this, the second of the machines before described, was employed.

Experiments  
with machine,  
No. 2.

Two hundred pieces of gold, of five different qualities, were employed in this experiment; twenty pieces of each kind were plain and smooth, the others were stamped with the die already mentioned. The two hundred pieces were mingled, and were enclosed within the cubic box.

Different quali-  
ties of the gold.

The following were the qualities of the gold. 1. Gold of 23 car.  $3\frac{1}{4}$  grs. 2. Gold made standard by silver. 3. Gold made standard by silver and copper. 4. Gold made standard by fine Swedish copper. 5. Gold made standard by equal parts of fine Swedish copper and dollar copper.

Their loss.

After 71720 revolutions of this machine, performed in 40 hours, the loss sustained was found to be as follows: of No. 1.  
the

the unstamped pieces 92,8 grs. stamped, 95,6 grs. No. 2, unstamped, 63,5 grs. stamped, 60,1 grs. No. 3, unstamped, 12 grs. stamped, 11,7 grs. No. 4, unstamped, 18 grs. stamped, 19,2 grs. No. 5, unstamped, 13 grs. stamped 12,1 grs. The total weight of the unstamped pieces, before friction, was 13701,3 grs. Their total loss, 199,3: the weight of the stamped, 13679,5; the loss 198,7.

All the pieces appeared to have suffered more on the edges Edges worn most, than on the faces; and those which were stamped had the impression more or less obliterated or flattened, in proportion to their respective degree of ductility, or to the loss which according to the result of this experiment, they had relatively sustained.

The different pieces, after the experiment, had a curious appearance; for, on the edges, which were become round and polished, a small regular raised bead or moulding was formed, which surrounded each face, like a frame; and both a raised bead round the edges; the faces concave. the faces concave.

The original diameter of the pieces was also diminished, The diameter diminished. nearly according to their different degrees of ductility, and according to the loss which they had experienced in consequence of the operation.

The measure of the diameters of the pieces, after the experiment, was,

Gold 23 car.  $3\frac{1}{4}$  grs. eight-tenths of an inch and  $\frac{3}{10}$ .

Gold alloyed with silver, nine-tenths of an inch.

The others varied little from nine-tenths and  $\frac{1}{10}$ ; which was less, by about  $\frac{1}{10}$  of an inch, than the original diameter of the pieces; and it was evident, that the pieces of fine gold, and those consisting of gold alloyed with silver, being the most ductile, had suffered the greatest loss, and were most diminished in diameter. Upon the whole, therefore, this experiment appears to corroborate what has been asserted concerning the former, *viz.* that soft or ductile gold suffers the greatest loss, when exposed to friction in contact with gold which is comparatively harder. These experiments for ascertaining the effects arising from the friction of coin against coin being gone through, another series was commenced with the apparatus, No. 3, by means of which various pieces were exposed to the action of certain powders and filings of metals, Apparatus, No. 3. which were separately sprinkled upon the horizontal table.

The

The pieces were properly fixed in their respective sockets and frames, and were placed so as to bear upon the table, with or without additional weights.

The table was moved by a wheel and pinion, so calculated as to avoid too rapid a motion; and the revolutions were denoted, as in the former experiments, by means of a counter.

Friction of gold  
by whiting,  
sand, filings of  
standard gold,  
and of iron.

The table was covered with fine powdered whiting, with fine white writing sand, with filings of gold made standard by copper, and lastly filings of iron. The last three were fixed on the table by means of a solution of isinglass. Gold of different kinds as before, standard silver, and fine copper, both stamped and unstamped, were subjected to the different trials.

General results.

From the whole of the preceding experiments, made with the three different machines, viewed and compared together, the author infers.

1st. That when equal friction, assisted by a moderate pressure, takes place between pieces of coin of a similar quality, abrasion is most commonly produced in an inverse ratio to the ductility.

2d. That the contrary effect happens, when pieces of different qualities rub against each other; for then, the more ductile metal is worn by that which is harder.\*

3d. That earthy powders and metallic filings produce similar effects, and tend to wear the different kinds of gold in proportion to their respective degrees of ductility.

Fine gold loses  
its impression.

Fine gold, being extremely soft and ductile, sustains a considerable loss, under many of the general circumstances of friction; and as at all times it appears certain, that the impressions which have been stamped upon it are most easily obliterated, even when actual abrasion does not take place, there is much reason to conclude, that gold of such extreme ductility is not that which is the most proper to be formed into coin.

Very hard gold.  
improper for  
coin: Why.

But gold of the opposite quality, or at least so hard as to be just capable of being rolled and stamped, seems to be equally improper for the purpose of coin. For, even sup-

\* Some experiments made at Paris, in 1790, upon pure and upon alloyed silver are concisely mentioned, the results of which appear to be nearly the same as those of the present experiments upon gold.

posing

posing that hard gold suffered, in every case, less by friction than that which is moderately ductile, (which is not however the fact,) and allowing that standard gold may, by a mixed alloy, be rendered as hard as gold reduced by copper to 18 carats, without changing the standard proportion of gold, yet it would be very difficult always to make such standard gold of an uniform degree of hardness. Moreover, by some experiments which Mr. H. purposely made at the Mint, upon the rolling and stamping of gold of different qualities, it evidently appeared, that gold equal in hardness to that of 18 carats, could not be employed with advantage; for, the additional labour which was required for the rolling and stamping of this hard gold, the frequent failure in making the impression, and the battering and breaking of the dies, fully proved, that the expence and difficulty attending the working of such gold, would by no means be compensated by any small degree of durability which it might possess over any other.

The extremes of ductility and of hardness being therefore equally objectionable, it follows of course, that gold of moderate ductility must be that which is the best adapted for coin; and, as nothing but silver or copper can be employed to alloy gold which is intended to be coined, it may be here observed, that whatever might have been the original motive for introducing the present standard of 22 carats, yet it appears, from the experiments lately described, that this proportion of  $\frac{1}{12}$  of the above-mentioned metals, is (every circumstance being considered) the best, or at least as good as any, which could have been chosen.

There is, however, some difference in the quality of gold, when alloyed with the standard proportion of silver, of silver and copper, and of copper, which requires to be considered.

Gold alloyed with one-twelfth of silver, is of a fine but pale yellow; it is very ductile; it is easily rolled, and may be stamped without being annealed; it consequently does not require to be blanché; and, after the complete process of coining, the surface and every part remains of an uniform quality, so that, by wear, it does not appear of different colours.

These properties are certainly much to be valued; but the objections to this kind of standard gold are,

Gold of moderate ductility best.

Gold alloyed with silver, its advantages;



its disadvantages ;

1st. The additional expence attending the use of silver as an alloy.

2d. The extreme pale yellow colour. And,

3d. That, from its great ductility, it is almost as liable to have the impressions which have been made upon it obliterated, as those which have been made upon fine gold.

All things being therefore considered, gold alloyed only with silver, does not appear to be so proper for coin as may at first be imagined.

with equal parts of silver and copper ; its advantages ;

Gold made standard by a mixture of equal parts of silver and copper, is not so soft as gold alloyed only with silver ; neither is it so pale, for it appears to be less removed from the colour of fine gold than either the former or the following metal.

Gold alloyed with silver and copper, when annealed, does not become black, but brown ; and this colour is more easily removed by the blanching liquor, or solution of alum, than when the whole of the alloy consists of copper. It may also be rolled and stamped with great facility ; and, under many circumstances, it appears to suffer less by friction, than gold alloyed by silver, or by copper alone.

its disadvantages :

But, after it has been subjected to the ordinary friction which must take place during the circulation of money, it is liable to appear of a deeper colour in those parts which are prominent, and are consequently the most exposed to friction. This defect arises from a cause which will soon be explained, but it cannot be regarded as an objection of any weight.

alloyed with copper alone.

The last kind of standard gold which remains to be mentioned, is that which is alloyed only by copper. This is of a much deeper colour than those which have been hitherto noticed, and it is slightly harder than either of them ; but nevertheless it is very ductile, provided that the copper be pure. It requires to be annealed, and then becomes nearly or quite black : which colour is not so easily removed by the blanching liquor, as that which is produced by the process of annealing, upon gold alloyed with a mixture of silver and copper.

It suffers less by many of the varieties of friction, than gold which is alloyed with silver ; but, in some cases, it seems to wear rather more than gold alloyed with silver and copper ; the difference is not however very considerable.

This

This sort of standard gold, as well as that which is alloyed with silver and copper, appears commonly, after a certain degree of wear, of a coppery colour, more or less deep, in those parts which are the most prominent; and, when coin thus alloyed exhibits such an appearance, it is frequently and vulgarly said to have been in contact with copper money; and sometimes guineas having this appearance have been refused, upon the supposition that they were debased. But the real fact is, that when copper constitutes part or the whole of the alloy, it becomes oxidized or calcined upon the surface of the blanks, by the process of annealing; and the blackish crust of copper, in this state, must then be removed by the solution of alum, called the blanching liquor. Now it is evident, that after this operation, the surfaces of the blanks or un-stamped pieces, can no longer be regarded as standard gold. For, if copper alone forms the alloy, it must be dissolved and separated from the surface of each piece of coin; and the same effect must also take place, with respect to the copper, in the alloy formed of copper and silver. So that, in the first case, each piece, when blanching, will consist of gold made standard by copper, covered with a thin coat of fine gold; and, in the second case, each piece will be composed of gold made standard by silver and copper, coated with gold alloyed with  $\frac{1}{24}$  of silver, or with half of the standard proportion of alloy, supposing the silver and copper to have been in equal quantities. As, therefore, the standard gold of which the pieces consist is always, more or less, of a deeper colour than the coating or film of the finer gold which covers each piece, it must be evident, that when this coating has been rubbed and removed from the raised or prominent parts, these will appear of a very different and deeper colour than the flat part or ground of the coin. The reason therefore is sufficiently apparent, why gold which is alloyed with silver only, cannot be liable to this blemish.

Upon a comparison of the different qualities of the three kinds of standard gold which have been lately mentioned, it appears, (strictly speaking,) that gold made standard by silver and copper is rather to be preferred for coin; but, as gold made standard by copper alone is not very much inferior in its general properties, it may be questioned, whether the few advantages which are thus gained, will compensate

Comparison of  
the three kinds.

penfate the additional expence of the filver required for half of the alloy; and, indeed, any extraordinary addition of filver appears to be the lefs neceffary, as there is commonly fome filver in the gold which is fent to the Mint, which, being reckoned as part of the alloy, contributes to produce thofe beneficial effects which refult when filver is purpofely added.

Extraordinary  
lofs fufained by  
our gold coin not  
imputable to fair  
wear.

From a general view of the prefent experiments, there does not appear to be any very great or remarkable difference in the comparative wear of the three kinds of fandard gold, all of which fuffer abraffion flowly, and with much difficulty; and (as it has been already obferved) the difference of wear between the two laft mentioned, is certainly but inconfiderable. For thefe reafons, and from the confideration of every other circumftance, it muft be evident, that the extraordinary lofs which the gold coin of this kingdom is ftated to have fufained within a certain limited time, cannot, with even a fhadow of probability, be attributed to any important defect in the compofition or quality of the fandard gold; and all that can be faid upon this fubject is, that fome portion of this lofs may have been caufed by the rough impreffion and milled edge now in ufe, by which each piece of coin acts, and is acted upon by the others, in the manner of a file.

The lofs thus occafioned cannot however be confiderable; for the quality of the prefent fandard gold is certainly that which is well adapted to refift abraffion, efpecially in the cafe of the fricftion of coin againft coin; and this is ftrongly corroborated by the obfervations of bankers and others, who are in the habit of fending or receiving large quantities of gold coin from any confiderable diftance. When a number of guineas, rather loofely packed, have been long fhaken together by the motion of a coach or other carriage, the effects of fricftion are obferved chiefly to fall upon only a few of the pieces. But it is not a little remarkable, that although thefe are often reduced nearly or quite to the ftate of plain pieces of metal or blanks, yet, upon being weighed, they are found to have fufained little or no lofs; and from this it appears, that the impreffions have been obliterated, not by an actual abraffion of the metal, but by the deprefion of the prominent parts, which have been forced into the mafs, and become reduced to a level with the ground of the coin. Pieces of hard gold would not fo eafily fuffer by depref-

sion; but the real loss would probably be greater, they being, in the case of the friction of coin against coin of similar quality, more susceptible of abrasion.

Upon the whole, there is every reason to believe, that our gold coin suffers but little by friction against itself; and the chief cause of natural and fair wear probably arises from extraneous and gritty particles, to the action of which the pieces may occasionally be exposed in the course of circulation. But still it must be repeated, that the united effects of every species of friction to which they may be subjected, *fairly and unavoidably*, during circulation, cannot produce any other wear than that which is extremely gradual and slow, and such as will by no means account for the great and rapid diminution which has been observed in the gold coin of this country.

## SECTION II.

### ON THE SPECIFIC GRAVITY OF GOLD WHEN ALLOYED BY VARIOUS METALS.

Difficulties in ascertaining the specific gravities of bodies, particularly from the inaccuracies of balances, the application and temperature of water, and the porosity of the object itself. Metals vary in their density by casting in a mould, by speedy or slow cooling, and by hammering.

Hammering and rolling is an imperfect remedy, and not applicable to the brittle metals.

The effects of alloys on the specific gravity of gold are very intricate, and only capable of being determined by a direct trial: for a numerous series of experiments clearly proved, only that the specific gravity of the compound may differ from the mean of the component parts, but that the effect of the same alloy, instead of being proportionate to the quantity employed, may differ considerably from this. To the peculiar effects produced by certain proportions of some of the metals, must be added the effects peculiar to certain compound alloys, whence arises an immense complicated series of alterations in specific gravity, never yet investigated by philosophers.

With regard to the expansion or contraction of the compound, little alteration appears to be produced by alloying gold with  $\frac{1}{12}$  of pure silver, as it produced only an expansion of 0,10. With copper it was 0,66: with equal parts of silver and copper; per

Iron.

Contraction by tin, bismuth, zinc, and antimony.

Mistake of Briffon in the alloy of gold with copper.

Specific gravity of a mixed metal affected by various circumstances. Differences in the same bar. Whence.

Unequal diffusion of alloy.

Mixed metals separate under fusion.

Long continued friction lessens specific gravity of metals. True chemical combinations.

per the expansion was 0,67 ; with iron it was 0,44 ; with iron and copper 0,37. Tin, bismuth, zinc, and antimony, produced a contraction. Lead and bismuth very much resemble each other in their effects on gold, and in the irregularity of these in various proportions.

Mr. Briffon has observed, that on alloying gold with  $\frac{1}{12}$  of copper, a mutual penetration takes place ; but in the course of the present experiments the reverse of this was found. It is probable, therefore, that his experiment was made with part of a large bar or ingot ; as the unequal diffusion of the alloy, the quantity of the metal, and the nature, form and position of the mould, are all capable of affecting the specific gravity. Thus, when the mixture is perfect, the bottom of a bar cast in a vertical mould will be of the greatest specific gravity, owing to the pressure of the superincumbent metal, while the quality of both ends appears equal by the assay. On the contrary, if the mixture be imperfect, the lighter metal flowing out of the crucible first, will render the bottom of the bar inferior both in quality and in specific gravity, as was found by experiment.

This unequal diffusion of the alloy through the mass, the exact distribution of which is not so easy as may be imagined, particularly in large quantities, is the most frequent cause of the variation in the specific gravity of standard gold. The difficulty has been considered, and an allowance made for it, called the remedy for the master of the mint. Even when metals have been completely mixed, if they be kept in fusion under certain circumstances, a separation, more or less perfect, sometimes takes place. This separation appears to be according to the relative affinities and specific gravities of the two metals, and is the soonest effected when the metals have not been perfectly mixed\*.

Beside the causes mentioned there is another, that occasions variations more or less considerable in the specific gravity of

\* Some compound metals may perhaps be mere mechanical mixtures ; but I am inclined to believe, that by much the greatest number are true chemical combinations ; and consequently, when these last have been properly formed, a separation of the component metals, by the means above-mentioned, can seldom if ever be effected. C. H.

metals,

metals, and does not appear to have been noticed. This is long continued friction, which always produced a diminution in the specific gravity of the pieces of metal exposed to it.

Among the other less powerful causes which produce some alteration in the specific gravity of gold, the processes of rolling, and of annealing, may also be enumerated; for, in the course of these experiments it appeared that the specific gravity of the bars, &c. was in a small degree increased by rolling, and that the contrary effect was produced by annealing.

Rolling increases it:  
Annealing diminishes it.

The specific gravity of gold, 23 car.  $3\frac{1}{4}$  grs. fine, when rolled and stamped without being annealed, was found to be 19,277; but, when the same was annealed, the specific gravity was 19,231, after stamping.

Mr. H. is however, inclined to believe, that annealing had reduced the specific gravity to much less than is here stated; and that the subsequent operation of stamping had, in some measure, compensated the effects of annealing. For, in the experiments lately mentioned, it was proved, that the specific gravity of the pieces which had not been annealed, was reduced, by long continued friction, from 19,277 to 19,171; an effect surpassing that which resulted from annealing by ,060 ( $19,231 - 19,171 = ,060$ ;) and, if heat was the cause, the reverse might have been expected, inasmuch as the annealing heat exceeded that which was produced by friction; but, as this was not the case, he is induced to be of opinion, that the specific gravity was again increased, by the subsequent stamping of the annealed pieces.

In addition, therefore, to those causes of variation in specific gravity which are the immediate consequences of hydrostatical operations, such as, the different height of the column of water, and the changes of temperature to which it is exposed during the experiments, the following, as far as they concern metallic substances, may be enumerated.

Causes of variation in specific gravity enumerated.

1. Imperfections in the interior of the mass, which are produced during the processes of melting and casting.

2. The difference of density in parts of the same mass, resulting from the quality and quantity of the metal, from the nature of the mould, from the more or less vertical position of it, and from the height of the column or bar of metal which is cast.

3. The

3. The unequal distribution of the metal, or metals, employed as an alloy, throughout the mass intended to be alloyed.

4. The peculiar effects which certain metals produce, when used singly or conjointly as alloys, and which are very different from the results of calculation.\*

5. Heat, whether produced by friction or excited in any other manner.

Absolute precision not to be expected.

As, therefore, the specific gravity of metals is liable to be influenced by such a variety of causes, it is almost in vain to expect absolute precision in the results of experiments made by different persons; but, at the same time, it may be observed, that by proper care and attention to the above circumstances, a degree of accuracy may be attained, sufficient to answer almost every useful purpose, although, from what has been said, it must appear improper to form opinions upon small fractional variations. By the experiments which were made, with every possible precaution, upon separate and intire ingots of gold, reduced to standard by silver, by silver and copper, and by copper alone, when cast in an iron mould like a cupel, it appeared, that the specific gravity of each of these kinds of standard gold is as follows.

Specific gravity of standard gold of different kinds.

That of our standard gold must occasionally differ.

Gold made standard by silver	-	-	-	-	17,927
Gold made standard by silver and copper	-				17,344
Gold made standard by copper	-	-	-	-	17,157

Now, as our gold coin commonly contains silver as part of the alloy, and as at different times this proportion of silver must have been various, and even considerable, particularly when the gold of Portugal, which is alloyed with silver, was brought to the Mint, it naturally follows that, exclusive of the many other causes of variation which have lately been enumerated, the specific gravity of our standard gold must occasionally be different,

\* There can be no doubt but that the effects of compound alloys are, in general, very different from those of each metal separately considered; and that such metallic combinations or compound alloys, like neutral salts, and many other compounds, have peculiar properties, which act variously upon the metals to which these compound alloys are added. A great number of accurate experiments are, however, requisite to elucidate a question so intricate.

It may here be also observed, that the peculiar properties of compound alloys, prove them to be real chemical combinations. G. H.

according

according to the relative proportions of silver and copper which compose the alloy;\* and, as the specific gravity of gold made standard by silver is, in the ingot cast under the above circumstances, 17,927, while that of gold made standard by copper is only 17,157, so, according to the relative proportions of these two metals, when united in the alloy, the specific gravity of the standard gold may vary between the two extremes of 17,927 and 17,157, although the real quality or value of the standard gold remains unchanged; and indeed, when some allowance is also made for small variations arising from other causes, the range of the different specific gravities of gold made standard by silver and copper, may be considered as nearly extending from 18 to 17.

There is much reason to believe, that the specific gravity of fine gold has been too highly estimated; and hence a notion has been too commonly received in this country, which has injuriously and unjustly been believed on the continent, that the standard gold of the present reign is inferior to that of the reigns preceding it. But the real fact is precisely the reverse. If a few of the old coins have proved better than standard, they were much inferior in the aggregate.

Specific gravity of fine gold estimated too high. Mistaken notions injurious to our coin.

Our old coin not so good as the new.

## II.

*A Memoir on the Appearance of Spectres or Phantoms occasioned by Disease, with Psychological Remarks. Read by NICOLAI to the Royal Society of Berlin, on the 28th of February, 1799.*

**P**HILOSOPHERS divide the human being into body and mind, because the numerous and distinct observations we make on ourselves oblige us to consider man particularly, as well in respect to his corporeal as his mental functions. Other philosophers have supposed that this subject might be treated with greater perspicuity by considering man as composed of body, soul, and mind. There can be no doubt but that these, and even more divisions might be invented. Such philosophers,

On the philosophical divisions respecting body and mind.

\* The first guineas which were coined, or those of CHARLES II. and JAMES II. were generally alloyed with standard silver; but the coins of the subsequent reigns have been alloyed with copper, added to compensate the deficiency of alloy, or of silver in the gold.



They are of  
little use,

however, have by no means considered that arbitrary systematic divisions, do not constitute an investigation of nature, and that philosophy often becomes more uncertain the more precisely we endeavour to distinguish and separate what nature has closely united. Sub-divisions in speculation seem as necessary as fences in fields, both are in themselves unproductive, and the more they are multiplied and extended the greater is the diminution of the fertility.

and quite inaccurate.

For my part, I will confess, that I do not know where the corporeal essence in man ceases, or where the mental begins; though I admit of the distinction, because the extreme differences can be clearly perceived. If we divide man into three parts, we shall be far from removing the difficulties, as we should be were we even to follow those modern philosophers, who regard the thinking subject alone as the real Being (*Ego*), and consider all external appearances as confined to the ideas of conscious beings. The greatest and most peculiar difficulties in the philosophic knowledge of the human subject consists in this, that we have never yet been able clearly and distinctly to ascertain the internal association of those striking differences which we observe in our being. Neither the most subtil physiology nor the finest speculative philosophy, have yet been able to explain the union of thought and physical operations. We may indeed doubt whether the labours of our German philosophers, though founded jointly upon modern speculation and modern chemistry, will be attended with any greater success. Extreme caution is most undoubtedly requisite to prevent our becoming too intimately and habitually acquainted with certain hypothetical notions respecting things really unknown, so as to mistake them for truths and deduce erroneous conclusions.

Speculations on  
the nature of  
thought are  
delusive.

It is much to be feared that the hypotheses and postulates of speculation will be of little value in this case; though to us they may seem very consistent and clear, while we regard them only in a certain point of view. An attention to experimental proof may bring us nearer to our aim, though its perfect accomplishment will perhaps never be within the reach of human investigation. Experiments or facts may shew the corporeal as well as the mental functions in several lights, and in such as we never can perceive by mere speculation.

Advantages of  
research.

Though it is truly said that the first principles of nature are placed beyond our reach, yet an endeavour to penetrate into  
the

the interior of nature will always prove beneficial to the human mind; as long as we do not presume to have completely investigated the subject; but continue our exertions by uniting the observations of facts with deliberate reasoning.

Since men have forgotten that what philosophy has separated is not on that account separated in nature, and since from the earliest ages, the mind and body of man have been considered as if distinct from each other, numberless questions have arisen which have given room for much controversy, without having met with any satisfactory answer. For example: Whether after the dissolution of the body, the spirit (or mind) continues to exist without the body? Whether the spirit can act without the body, and in what manner? And lastly, it is also a question, Whether, as we consider a disembodied spirit not only in a state of separate continual existence, but also in a state of continual existence and continual action *amongst us*, a mere spirit and its actions cannot become perceptible to our senses?—Whether the figure of a spirit (and in particular that of a deceased person) may not be seen? and, Whether a sound proceeding from it may not affect the ear of the living? All the knowledge usually considered as possible to be had of a departed spirit is confined to seeing and hearing; for as far as my information extends, the devil is the only spirit that enjoys the privilege of affecting the sense of smell at his departure.

We have less motive for disputing about the absolute possibility of seeing a spirit, because the idea of a spirit is so indistinct and vague, and because the words spirit and body in considering man, do in reality indicate mere relative notions. It is inconsistent with every known law of nature to suppose that those terms of relation adopted by us solely for the purpose of investigating the nature of man do themselves possess any separate and independent existence. This argument causes a suspicion of deceit or imposition always to attach to narratives of the apparitions of disembodied spirits. But those who are inclined to see and hear spirits, are not satisfied with this summary solution; they appeal to experience, against which no maxim *a priori* can hold. This only is required, that the experience must be true and well attested.

Individuals who pretend to have seen and heard spirits are not to be persuaded that their apparitions were simply the creatures of their senses. You may tell them of the impositions

The hypothesis of body and mind has led to many disquisitions respecting ghosts or spirits.

Why the narratives of apparitions are generally concluded to be impostures.

But the delusions of imagination or disease well deserve to be investigated.

that are frequently practised, and the fallacy which may lead us to take a spirit of our imagination by moon-light for a corpse. We are generally advised to seize the ghosts, in which case it is often found that they are of a very corporeal nature.—An appeal is also made to self-deception, because many persons believe they actually see and hear where nothing is either to be seen or heard. No reasonable man, I think, will ever deny the possibility of our being sometimes deceived in this manner by our fancy, if he is in any degree acquainted with the nature of its operations. Nevertheless, the lovers of the marvellous will give no credit to these objections, whenever they are disposed to consider the phantoms of imagination as realities. We cannot therefore sufficiently collect and authenticate such proofs as shew how easily we are misled; and with what delusive facility the imagination can exhibit, not only to deranged persons, but also to those who are in the perfect use of their senses, such forms as are scarcely to be distinguished from real objects.

Striking instances of apparitions seen by the author.

I myself have experienced an instance of this, which not only in a psychological, but also in a medical point of view appears to me of the utmost importance. I saw, in the full use of my senses, and (after I had got the better of the fright which at first seized me, and the disagreeable sensation which it caused) even in the greatest composure of mind, for almost two months constantly, and involuntarily, a number of human and other apparitions;—nay, I even heard their voices;—yet after all, this was nothing but the consequence of nervous debility; or irritation, or some unusual state of the animal system.

The publication of the case in the *Journal of Practical Medicine*, by Professor Hufeland of Jena, is the cause of my now communicating it to the Academy. When I had the pleasure of spending a few happy days with that gentleman last summer, at Pyrmont, I related to him this curious incident.

Narrative and remarks on spectres and apparitions.

But as it is probable he might not distinctly remember that which I had told altogether accidentally, perhaps indeed not very circumstantially, some considerable errors have been admitted into his narrative. In such a case, however, it is more necessary than in any other, to observe every thing with accuracy, and to relate it with fidelity and distinctness. I shall therefore pass over nothing which I remember with any degree of certainty. Several incidents connected with the apparitions

partitions seem to me of great importance; though we might be apt to regard them in a secondary point of view; for we cannot determine of what consequence even a circumstance of the most trivial nature may be, if at any future period (in case more experiments of a like nature are ascertained) some suppositions or conclusions can be made respecting the origin of such phantoms, or on some law of the association of ideas according to which they are modified or follow one another.

Narrative and remarks on spectres produced by nervous indisposition.

I was also, which is seldom the case, in a situation to make observations on myself. I took down therefore in a few words what was most important, and recounted it immediately to several persons. My memory, which is extremely retentive, has besides treasured up the most minute circumstances; the more on that account, as this story has very often proved the subject of my impartial consideration, not only with regard to my own particular situation but also in respect to its many psychological consequences. Its truth will, I hope, require no further assurance on my part, since a member of this academy (Mr. Selle) is an unexceptionable witness of it, having, as my physician, received a daily account of all that happened to me.

It would be extremely improper in an assembly like the present to speak much of myself; it can only be excusable in this particular case, where it serves to throw greater light on scientific investigation. I must request permission therefore to notice several particulars of my situation previous to my seeing the phantoms, as those incidents may have greatly affected the state of my body and mind during that time.

In the last ten months of the year 1790, I underwent several very severe trials, which greatly agitated me. From the month of September in particular, repeated shocks of misfortune had befallen me, which produced the deepest sorrow. It had been usual for me to lose blood by venesection twice a year. This was done once on the 9th of July 1790, but towards the close of the year it was omitted. In 1783 I had been suddenly seized with a violent giddiness, which the physician imputed to an obstruction in the small muscles of the *abdomen*, proceeding from too intense an application to study, and my sedentary manner of life for many years. These complaints were removed by a three years cure, and the rigid observance of a strict diet during that time. In the first stage of the malady the application of leeches to the *anus* had been particularly effective,

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fective, and this remedy I had from that time regularly applied twice or thrice a year, whenever I felt congestion in the head. It was on the 1st of March 1790 that the leeches had been last applied; the bleeding therefore and the clearing of the minuter blood-vessels by leeches had, in 1790 been less frequently observed than usual. A circumstance too that could not tend to benefit my deplorable situation was, that from September I had been continually engaged in business which required the severest exertion, and which, from frequent interruptions, was rendered still more burthensome and distressing.

In the first two months of the year 1791, I was much affected in my mind by several incidents of a very disagreeable nature; and on the 24th of February a circumstance occurred which irritated me extremely. At ten o'clock in the forenoon my wife and another person came to console me; I was in a violent perturbation of mind, owing to a series of incidents which had altogether wounded my moral feelings, and from which I saw no possibility of relief; when suddenly I observed at the distance of ten paces from me a figure,—the figure of a deceased person. I pointed at it, and asked my wife whether she did not see it. She saw nothing, but being much alarmed, endeavoured to compose me, and sent for the physician. The figure remained some seven or eight minutes, and at length I became a little more calm; and as I was extremely exhausted, I soon afterwards fell into a troubled kind of slumber, which lasted for half an hour. The vision was ascribed to the great agitation of mind in which I had been, and it was supposed I should have nothing more to apprehend from that cause; but the violent affection had put my nerves into some unnatural state, from this arose further consequences, which require a more detailed description.

In the afternoon, a little after four o'clock, the figure which I had seen in the morning again appeared. I was alone when this happened; a circumstance which, as may be easily conceived, could not be very agreeable. I went therefore to the apartment of my wife, to whom I related it. But thither also the figure pursued me. Sometimes it was present, sometimes it vanished, but it was always the same standing figure. A little after six o'clock several stalking figures also appeared; but they had no connection with the standing figure. I can assign no other reason for this apparition than that, though much more composed

composed in my mind, I had not been able so soon entirely to forget the cause of such deep and distressing vexation, and had reflected on the consequences of it, in order, if possible, to avoid them; and that this happened three hours after dinner, at the time when the digestion just begins.

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At length I became more composed with respect to the disagreeable incident which had given rise to the first apparition; but though I had used very excellent medicines, and found myself in other respects perfectly well, yet the apparitions did not diminish, but on the contrary rather increased in number, and were transformed in the most extraordinary manner.

After I had recovered from the first impression of terror, I never felt myself particularly agitated by these apparitions, as I considered them to be what they really were, the extraordinary consequences of indisposition; on the contrary, I endeavoured as much as possible to preserve my composure of mind, that I might remain distinctly conscious of what passed within me. I observed these phantoms with great accuracy, and very often reflected on my previous thoughts, with a view to discover some law in the association of ideas, by which exactly these or other figures might present themselves to the imagination.— Sometimes I thought I had made a discovery, especially in the latter period of my visions; but on the whole I could trace no connexion which the various figures that thus appeared and disappeared to my sight had, either with my state of mind, or with my employment, and the other thoughts which engaged my attention. After frequent accurate observations on the subject, having fairly proved and maturely considered it, I could form no other conclusion on the cause and consequence of such apparitions than that, when the nervous system is weak and at the same time too much excited, or rather deranged, similar figures may appear in such a manner as if they were actually seen and heard; for these visions in my case were not the consequence of any known law of reason, of the imagination, or of the otherwise usual association of ideas; and such also is the case with other men, as far as we can reason from the few examples we know.

The origin of the individual pictures which present themselves to us, must undoubtedly be sought for in the structure of that organization by which we think; but this will  
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always remain no less inexplicable to us than the origin of those powers by which consciousness and fancy are made to exist.

The figure of the deceased person never appeared to me after the first dreadful day; but several other figures shewed themselves afterwards very distinctly; sometimes such as I knew, mostly, however, of persons I did not know, and amongst those known to me, were the semblances of both living and deceased persons, but mostly the former: and I made the observation that acquaintance with whom I daily conversed never appeared to me as phantasms; it was always such as were at a distance. When these apparitions had continued some weeks, and I could regard them with the greatest composure, I afterwards endeavoured, at my own pleasure to call forth phantoms of several acquaintance, whom I for that reason represented to my imagination in the most lively manner, but in vain.—For however accurately I pictured to my mind the figures of such persons, I never once could succeed in my desire of seeing them *externally*; though I had some short time before seen them as phantoms, and they had perhaps afterwards unexpectedly presented themselves to me in the same manner. The phantasms appeared to me in every case involuntarily, as if they had been presented externally, like the phenomena in nature, though they certainly had their origin internally; and at the same time I was always able to distinguish with the greatest precision phantasms from phenomena. Indeed, I never once erred in this, as I was in general perfectly calm and self-collected on the occasion. I knew extremely well, when it only appeared to me that the door was opened, and a phantom entered, and when the door really was opened and any person came in.

It is also to be noted, that these figures appeared to me at all times, and under the most different circumstances, equally distinct and clear. Whether I was alone, or in company, by broad day-light equally as in the night time, in my own as well as in my neighbour's house; yet when I was at another person's house, they were less frequent, and when I walked the public street they very seldom appeared. When I shut my eyes sometimes the figures disappeared, sometimes they remained even after I had closed them. If they vanished in the former case, on opening my eyes again, nearly the same figures appeared which I had seen before.

I some-

I sometimes conversed with my physician and my wife, concerning the phantasms which at the time hovered around me : for in general the forms appeared oftener in motion than at rest. They did not always continue present—they frequently left me altogether, and again appeared for a short or longer space of time, singly or more at once ; but, in general, several appeared together. For the most part I saw human figures of both sexes ; they commonly passed to and fro as if they had no connection with each other, like people at a fair where all is bustle ; sometimes they appeared to have business with one another. Once or twice I saw amongst them persons on horseback, and dogs and birds ; these figures all appeared to me in their natural size, as distinctly as if they had existed in real life, with the several tints on the uncovered parts of the body, and with all the different kinds and colours of clothes. But I think, however, that the colours were somewhat *paler* than they are in nature.

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None of the figures had any distinguishing characteristick, they were neither terrible, ludicrous, nor repulsive ; most of them were ordinary in their appearance,—some were even agreeable.

On the whole, the longer I continued in this state, the more did the number of phantasms encrease, and the apparitions became more frequent. About four weeks afterwards I began to hear them speak : sometimes the phantasms spoke with one another ; but for the most part they addressed themselves to me : these speeches were in general short, and never contained any thing disagreeable. Intelligent and respected friends often appeared to me, who endeavoured to console me in my grief, which still left deep traces on my mind. This speaking I heard most frequently when I was alone : though I sometimes heard it in company, intermixed with the conversation of real persons ; frequently in single phrases only, but sometimes even in connected discourse.

Though at this time I enjoyed rather a good state of health both in body and mind, and had become so very familiar with these phantasms, that at last they did not excite the least disagreeable emotion, but on the contrary afforded me frequent subjects for amusement and mirth ; yet as the disorder sensibly encreased, and the figures appeared to me for whole days together, and even during the night, if I happened to awake,

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I had recourse to several medicines, and was at last again obliged to have recourse to the application of leeches to the anus.

This was performed on the 20th of April at eleven o'clock in the forenoon. I was alone with the surgeon, but during the operation, the room swarmed with human forms of every description, which crowded fast one on another; this continued till half past four o'clock, exactly the time when the digestion commences. I then observed that the figures began to move more slowly; soon afterwards the colours became gradually paler; every seven minutes they lost more and more of their intensity, without any alteration in the distinct figure of the apparitions. At about half past six o'clock all the figures were entirely white, and moved very little; yet the forms appeared perfectly distinct; by degrees they became visibly less plain, without decreasing in number, as had often formerly been the case. The figures did not move off, neither did they vanish which also had usually happened on other occasions. In this instance they dissolved immediately into air; of some even whole pieces remained for a length of time, which also by degrees were lost to the eye. At about eight o'clock there did not remain a vestige of any of them, and I have never since experienced any appearance of the same kind. Twice or thrice since that time I have felt a propensity, if I may be so allowed to express myself, or a sensation as if I saw something which in a moment again was gone. I was even surprised by this sensation whilst writing the present account, having, in order to render it more accurate, perused the papers of 1791, and recalled to my memory all the circumstances of that time. So little are we sometimes, even in the greatest composure of mind, masters of our imagination.

This is an exact narrative of the apparitions which I observed during the disordered state of my nerves: and I shall now add a few observations, partly with the intention of explaining their origin from other observations made on myself, and partly with a view of pointing out at least some distant psychological consequences, which might be deduced from this remarkable case.

Experience shews that we may, in various manners, imagine that we see figures, and even hear them when they do not really exist.

1st, And commonly this may happen in dreams.—The manner of dreaming is different in every individual, and probably depends on the joint effects of the powers of intellect, and those by which the impressions of the senses are received, and these are modified by the state of the system at each particular time. I have myself made some remarkable observations on the nature of my dreams, and compared them with some observations on that subject which have been communicated to me by others.

2d. In every degree of mental derangement till absolute insanity.

3d. In fevers of the brain, which for a short time, or at certain intermitting periods, occasion a delirium.

4th. By the mere power of imagination without any fever, when in other respects the judgment is perfectly sound. In this case it is very difficult to discover the truth, unless we combine an accurate habit of observation with the most impartial scrutiny.

Instances are too frequent in which we are imposed upon, not by the imagination, but by delusion of the judgment. How many are there, who prefer the marvellous and assume an air of importance, when they have an opportunity of relating wonderful things of themselves—How few are there who endeavour to divest themselves of prejudice, or to check their imagination; and still fewer are they who are accurate in their observations, especially in such as relate to themselves; even those who have sufficient firmness to adhere strictly to the truth form an inconsiderable number. Hence it is, that when a person relates any strange incident, he either detracts or magnifies, and will even fancy that he has verified some facts, which he has invented only at the moment that he relates them. This last is the case with a class of men who obstinately persist in their own opinions, and frequently assert more than they can support, merely with a view to maintain what they have once advanced. All the above mentioned circumstances seem to have coincided in the celebrated visions of Emanuel Swedenborg. He delighted in speculation and mystical theology; he had formed a system for himself in which ghosts were necessary, and it was his primary view to establish this wonderful system. It is possible that he may have seen phantasms, the more so as he studied

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studied much, and was a great eater.\* But in order to appear a prodigy to the world, he embellished his visions on which he wrote voluminous treatises, by creating new images in conformity with his own system.

Lastly, those who are most conversant with the marvellous, give but a very indistinct idea of their visions. This I have found in conversation with persons who in other respects were very worthy characters, but who were great admirers of the what are termed occult sciences, which they cultivated to such a degree, that to give you a notion of it here would seem preposterous. I have frequently discoursed on spirits, and the seeing of spirits, with a person who ranked very high in the school of secret wisdom, but who was otherwise a man of a very limited capacity, and rather ignorant in all those sciences which enlighten the mind. This person told me amongst other things that he should feel very unhappy, were he not continually in company with spirits. As I have always taken a pleasure in the clear development of human opinions, however absurd they may appear, I was desirous to learn in what manner he saw the spirits, and how he came into company with them?—But here he would not allow of the appearance of any corporeal forms; he assured me that spirits were only to be seen with the eyes of the spirit: then he added in a very serious tone, “Just as the human soul is *Nachhaefsch*, “or a branch taken off the tree, so are all spirits branched off “from the supreme spirit, as it in the astrigent motion com- “pressed its being.” On nearer enquiry I could easily perceive that he entertained a confused notion of the cabalistical ontology of Spinoza, and that he imagined all the powers in nature to be spirits. What he meant to say therefore, was neither more nor less than that he should feel unhappy, did he live in a world where nature was perfectly inanimate; if he could not think that every thing around him was in the continual and mutual *exercise* of its powers. In this belief then he peopled all space with spirits, nearly in the same manner as the antient mythology peopled the woods with Dryads and

\* On this subject, the review of Swedenborg's Works in the *Allgemeine Deutsche Bibliothek*, vol. 107, p. 15, is very interesting. In it the resemblance of Swedenborg's system, with the visions of the German enthusiast, Johann Tennhart, is clearly accounted for: he also was a great eater.

**Hamadryads.** Indeed, every thing properly considered, the opinion of my cabalist is not quite so very absurd as you may suppose; for in reality, the word *power* is with the philosopher only that which the  $x$  is to the mathematician; and, if I be not altogether mistaken, the mathematician can with his  $x$ , bring more clear truths to light, than the philosopher by the word *power*. If a given power cannot be rendered subservient to deduction, so that, like Newton's *calculus*, it shall perfectly accord with experience; nothing more will be determined or explained by the mere word *power*, than by the word *spirit*; and I doubt much whether the new judicious Kantian system of Dynamic natural philosophy, which considers all bodies as mere aggregates of powers, would not rather cut the gordian knot than unravel it.

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It is not very uncommon that by a derangement of the corporeal powers, even without insanity and inflammatory fevers, apparitions do strike the eye externally, which are only internally the production of the imagination. The experience of this may teach us a lesson of forbearance, not rashly to consider as impostors those well disposed persons who believe they have seen apparitions. But as manifold experience shews us how far the human imagination can go in the external representation of pictures; it may also admonish those well-disposed persons not to ascribe to their visions any degree of reality, and still less to consider the effects of a disordered system, as proofs that they are haunted by spirits.

The celebrated Justus Moser frequently believed that he saw flowers. Another of my acquaintance sees in like manner, at times, mathematical figures, circles, squares, &c. in different colours. More examples of this kind may perhaps be found in Moretz's Magazine, in Krueger's Experimental Psychology, and in Bonnet's Psychological writings. The hearing of sounds is a case which seldom occurs. My much-lamented friend Moses Meudeljohn had, in the year 1792, by too intense an application to study, contracted a malady, which also abounded with particular psychological apparitions. For upwards of two years he was incapacitated from doing any thing; he could neither read nor think, and was rendered utterly incapable of supporting any loud noise. If any one talked to him rather in a lively manner, or if he himself happened to be disposed to lively conversation, he fell

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fell in the evening into a very alarming species of catalepsy, in which he saw and heard every thing that passed around him, without being able to move a limb. If he had heard any lively conversation during the day, a stentorian voice repeated to him while in the fit, the particular words or syllables that had been pronounced with an impressive accent, or loud emphatic tone, and in such a manner that his ears reverberate.

Seldom as it may happen, that persons believe they see human forms, yet examples of the case are not wanting. A respectable member of this academy, distinguished by his merit in the science of botany, whose truth and credibility are unexceptionable, once saw in this very room in which we are now assembled, the phantasm of the late president Maupertuis. A person of a sound and unprejudiced mind, though not a man of letters, whom I know well, and whose word may be credited, related to me the following case. As he was recovering from a violent nervous fever, being still very weak, he lay one night in bed perfectly conscious that he was awake, when the door seemed to open, and the figure of a woman entered, who advanced to his bed-side. He looked at it for some moments, but as the sight was disagreeable, he turned himself and awakened his wife; on turning again however he found the figure was gone. But out of many cases I have never known an instance like my own, in which any person had for almost two months constantly beheld such visionary forms, and seemed even to have heard them; except it was that of two young ladies, who, as I have been credibly informed, frequently saw appearances of this nature.

I am by no means insensible to a certain feeling which admonishes me of the impropriety of talking so much of myself in an assembly like this; but since I transgress only with a scientific intention, to contribute to the knowledge of the effects of the human imagination, I must endeavour to suppress this feeling. I may look for pardon, I trust, from those who know and respect every thing which tends to enlarge the stock of human knowledge, even if I speak more of myself. For, when I proceed to describe the state of my imagination, and the nature of the apparitions during a previous malady, it will be merely with an intention to shew the apparitions which form the subject of this lecture in a less wonderful point of view,  
and

and by that means perhaps to contribute in some degree to the illustration of so strange an incident.

I must observe that my imagination possesses in general a great facility in picturing. I have for example sketched in my mind a number of plans for novels and plays; though I have committed very few of them to paper, because I was less solicitous to execute than to invent. I have generally arranged these outlines when, in a cheerful state of mind. I have taken a solitary walk, or when travelling I have sat in my carriage, and could only find employment in myself and my imagination. Constantly and even now do the different persons whom I imagine in the formation of such a plot, present themselves to me in the most lively and distinct manner: their figure, their features, their manner, their dress, and their complexion, are all visible to my fancy. As long as I meditate on a fixed plan, and afterwards carry it into effect,—even when I am often interrupted, and must begin it again at different times, all the acting persons continue present in the very same form in which my imagination at first produced them. I find myself frequently in a state betwixt sleeping and waking, in which a number of pictures of every description, often the strangest forms, shew themselves, change and vanish. In the year 1778, I was afflicted with a bilious fever, which, at times, though seldom, became so high as to produce delirium. Every day towards evening, the fever came on, and if I happened to shut my eyes at that time, I could perceive that the cold fit of the fever was beginning even before the sensation of cold was observable. This I knew by the distinct appearance of coloured pictures of less than half their natural size, which looked as if in frames. They were a set of landscapes composed of trees, rocks, and other objects. If I kept my eyes shut, every minute some alteration took place in the representation. Some figures vanished, and others appeared. But if I opened my eyes all was gone; if I shut them again I had quite a different landscape. This case was therefore entirely different from what afterwards in the year 1791, when the figure remained unchanged during the opening and shutting of the eyes. In the cold fit of the fever I sometimes opened and shut my eyes every second for the purpose of observation, and every time a different picture appeared replete with various objects

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jects which had not the least resemblance with those that appeared before. These pictures presented themselves without interruption, as long as the cold fit of the fever lasted. They became fainter as soon as I began to grow warm, and when I was perfectly so, all were gone. When the cold fit of the fever was entirely past, no more pictures appeared; but if on the next day I could again see pictures when my eyes were shut, it was a certain sign that the cold fit was coming on. I must further observe, that when I either think deeply on a subject, or write attentively, particularly when I have exerted myself for some time, a thought frequently offers itself which has no connection with the work before me, and this at times in a manner so very lively, that it seems as if expressed in actual words.

This natural vivacity of imagination renders it less wonderful, that after a violent commotion of mind, a number of delusive pictures should appear for several weeks in succession. Their leaving me on the application of leeches, shews clearly that some anomaly in the circulation of the blood was connected with the appearance of those phantasms; though it may perhaps be too hasty a conclusion to seek for their cause in that alone. It seems likewise remarkable, that the beginning of the apparitions, after the disturbance in my mind was settled, as well as the alteration which took place when they finally left me, happened exactly at the time when digestion commenced. It is no less remarkable, that the apparitions before they entirely ceased, lost their intensity of colours; and that they did not vanish or change as formerly, but seemed gradually to dissolve into air.

Had I not been able to distinguish phantasms from phenomena, I must have been insane. Had I been fanatic or superstitious, I should have been terrified at my own phantasms, and probably might have been seized with some alarming disorder. Had I been attached to the marvellous, I should have sought to magnify my own importance, by asserting that I had seen spirits; and who could have disputed the facts with me? The year 1791 would perhaps have been the time to have given importance to these apparitions. In this case however, the advantage of sound philosophy, and deliberate observation may be seen. Both prevented me from becoming either a lunatic or an enthusiast; with nerves so strongly excited

cited, and blood so quick in circulation, either misfortune might have easily befallen me. But I considered the phantoms that hovered around me as what they really were, namely, the effects of disease; and made them subservient to my observations, because I consider observation and reflection as the basis of all rational philosophy.

Our modern German philosophers, will not allow that observation ought to be admitted in theoretical philosophy. Hence arose Kants' Transcendental Idealism, which at last degenerated into the gross enthusiastic idealism; which is found in Fichte's writings. This philosopher considers all external objects as our own productions. "What we consider as things independent of us are," according to him, "no more than our own creatures, which we fear, admire and desire; we believe our fate to be dependent on a shadow, which the single breath of a free being might destroy." These are Mr. Fichte's own words\*.

The mere picture in the mind, without external experience, would never be sufficient to afford us a convincing proof, whether we saw phenomena or phantasms. The critical philosophers maintain, that knowledge deduced from observation is merely empirick, and therefore not to be depended on; it is perhaps true that nature has assigned us no greater certainty than this respecting our ideas. But could we be truly conscious of our grounds of reason, if the appearances called external, which follow laws that do not depend on the representations in our mind, did not continually agree with those representations? Are we possessed of any other criterion? Does not the great theoretical philosopher, when he sees every thing yellow, conclude that his eye is jaundiced; or when every thing appears black to him, that his brain is affected? In these cases he does not trust his imagination or mental powers alone.

I may here apply the consideration of the illusions which I witnessed. I am well aware that no general conclusions can be drawn from a single instance; but still the experience of a single case, if accurately observed and faithfully described, is sufficient to destroy hypotheses which have too long been honoured with the name of systems.

\* Fichte's Appeal, p. 44.



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According to Fichte, since during the situation I have above described, I was in other respects in the perfect use of my reason, as well as the persons who were really about me; as the apparitions which I saw, as well as those which are considered as realities, were the one as well as the other, my own productions:—Why then were my creatures of both kinds so essentially different?

My judgment shewed me this plainly, by conclusions founded on the previous course of observations. The greatest modern idealists who depend so much on the confusion in which they have involved themselves by the supposed depth of their speculations, will certainly never pretend that both perceptions were of the same nature; since if so, I could not have investigated their difference? But by what means could this be done? I observed that real persons followed in a determinate order, by external laws that do not depend on me, in an order that I myself must continually follow, as was evident from my sense of consciousness. I could also lay hold of the real objects, as well as of myself. Neither of these circumstances was, however, the case with the phantasms; I had always found it so in the constant observation of myself, of the apparitions without me, and in my own consciousness.

The phantasms, as well as the phenomena, no doubt, lay in my mind; but I am necessarily compelled to ascribe to the latter, the same reality which I am obliged to ascribe to myself: viz. something that does not lie in my mind alone; something that also exists without my mind; something independent of my consciousness, which determines the nature of my idea; something which we formerly used to call *the thing itself*, before the critical philosophy so unjustly reprobated this unexceptionable term. On the contrary, however, I could not ascribe this same reality to the illusion; I could form no other conclusion, than that they originated in my internal consciousness alone; in a consciousness which was also disordered, as I might justly conclude from the observations I made on myself. I repeat, that both the phenomena and the phantasms existed in my mind: if I had not been able to distinguish between them, I must have been insane. By what means could I distinguish, if I did not attribute reality to the former;—and that they possessed reality, I inferred from ob-  
servations

servations to which I am still inclined to give confidence, until Mr. Fichte can more clearly convince me that it ought in no case to be depended on.

### III.

*Analysis of Ambergris; by Cit. BOUILLON LA GRANGE\*.*

IT is an opinion now generally adopted, that ambergris is Ambergris found in the stomach of the physfeter macrocephalus. formed in the stomach of the cachalot, or spermaceti whale, *physfeter macrocephalus*, and appears to be a product of its digestive faculties.

Dr. Swediaur has shewn, in his inquiries into the nature and origin of ambergris, that the beaks of the cuttlefish, interspersed throughout all the large pieces of ambergris, that are found swimming on the sea, or cast upon the shore, as well as those extracted from the bellies of whales, belong to the species called by Linneus *sepia octopodia*. The existence of these beaks and other foreign substances in ambergris evidently proves it to have been originally in a soft or fluid state. Dr. Swediaur asserts, that the whale, in the belly of which ambergris is found, is the same species as that from which spermaceti is extracted, which appears to be the *physfeter macrocephalus* of Linneus; and feeds chiefly on the large species of cuttlefish. The ambergris is found in the intestinal canal of this fish; it is a source of disease to it †; and after it issues from the cavity in which it had been included, it gradually acquires the solidity it is known to possess. Beaks of cuttlefish found in all the large pieces of ambergris. whence it must once have been soft, if not fluid. The spermaceti whale that produces it. Occasions a disease in the fish? becomes solid after its exclusion. Found on the shores of the Indian Ocean and its islands.

Ambergris is found in the Indian Seas, near the Moluccas, Maldivia Islands, and Madagascar, on the coasts of China and Japan, and from Jolo to the Philippine Islands. It is frequently collected on the shores of the Island of Maragnan, or of Brazil; but more commonly on those of Africa, toward cape Blanco, the gulf of Arguin, the bay of Portendie, and on some other Islands, that extend from Mosambique to the Red Sea. Of Brazil, and also of Africa.

\* *Annales de Chimie*, No. 139. or XLVII. 68.

† Is it not rather the effect, than the cause of disease? J. C.

The inhabitants of the Samballas seek for it on the shore after storms by the smell.

Certain birds and other animals fond of it.

Certainly a vegetable production.

Excrements of some animals, particularly of the ox and pig, resemble it in smell.

Cowdung called in some places native musk.

External qualities of ambergris.

Its odour more powerful as it grows old, or when mixed with other perfumes.

Marks of good ambergris.

The older chemists classed it among the bitumens.

Geoffroy's analysis of it by alcohol,

and by distillation.

From the accounts of various travellers, the inhabitants of the Samballas seek for it in a singular manner: they hunt it by scent. After a storm they run along the shore, and if any ambergris be thrown up, they find it by the smell. There are certain birds and other animals on those coasts, that are very fond of ambergris, and, attracted from a distance by its smell, they search for it to eat.

There is no doubt, that ambergris is a vegetable production. Many substances resemble it greatly in smell, such as the excrements of mammiferous animals, particularly those of the ox and the pig. I have found, that cowdung dried in the sun, has a smell much like that of ambergris, and even of musk, whence in some countries this substance, so prepared, has received the name of *native musk*.

Ambergris, *ambra grisea*, is a light substance, swimming on water, solid, opaque, of an ashen gray colour streaked with white and yellowish brown, slightly odoriferous, its odour displaying itself more as it grows old, or when it is mixed with musk or other aromata, as is done in preparing perfumes or odoriferous waters.

In its natural state good ambergris is known by adhering like wax to the edge of a knife with which it is scraped, retaining the impression of the teeth or nails, and emitting a fat odoriferous liquid on being penetrated with a hot needle. Though solid, and in general brittle, it is not hard enough to take a polish; but on rubbing it with the nail it becomes as smooth as hard soap.

Geoffroy, Neumann, Grim, and Brow, have classed ambergris among the bitumens. The analysis made of it by these chemists was inadequate to determine its nature. Ambergris, says Geoffroy, melts into a resin of a yellow or gold colour; kindles, and burns with flame. Spirit of wine does not dissolve it entirely; a black substance like pitch being left, on which it does not act. When it is dissolved, it lets fall after some time a white cloudy sediment, which gradually coagulates, and grows thicker and thicker. This *coagulum*, on drying, changes to a shining foliated earth, nowise different from spermaceli.

On distillation, according to the same chemist, ambergris yields at first an insipid phlegm, then an acid spirit or liquor and

and a very odoriferous yellow oil, with a small portion of a volatile acidofaline salt; and lastly, a shining black bituminous substance remains at the bottom of the retort. Hence we see this analysis, which does not differ from those related by all other chemists, requires to be revised, in order to give us determinate ideas of the nature of this singular substance.

This analysis is sufficient.

It is perhaps necessary to apprise those, who wish to repeat these experiments, that they should pay great attention to the choice of the ambergris. Many varieties are found in the shops, the different kinds of which are distinguished by their price. No doubt this substance is fabricated, as callor is in some parts of Germany. Bayen assured me, that he had seen it made at Frankfort; and it is well known that this father of chemistry saw clearly, and that his memory was not apt to deceive him; and, what is very rare among travellers, that he never told a lie.

Necessary to be very careful in choosing it. Many varieties of it in the shops

Fabricated by art, as Bayen saw at Frankfort

I have examined several specimens of the ambergris of the shops: some varied in specific gravity, were more or less deep in colour, had very little smell, and were flexible; others were of an ashen gray colour, and tolerably hard; and some were almost stony, scarcely at all soluble in alcohol, and void of smell.

Differences of these varieties.

The ambergris I analysed was not purchased from the shops; and, on comparing it with that in the cabinet of the Museum, I could find no difference, either in colour or in smell.

### *Physical Properties.*

It is of an ashen gray colour, internally variegated with a few yellow streaks, of a sweet and pleasing smell, softening between the fingers; when reduced to a fine powder it is of a deeper colour; pounded in a glass mortar it agglutinates, and adheres to the pestle.

Its colour, smell, texture,

Of a flat and almost insipid taste, exhibiting the same appearances as wax when chewed between the teeth.

Its specific gravity is to that of water as 844 or 849 to specific gravity. 1000.

According to Briffon, the specific gravity of ambergris is 9263; the weight of the French cubic inch, 4 gros 58 grs.; that of the cubic foot, 64 lbs. 14 oz. 3 gr. 47 grs. \*

\* The specimens of ambergris, on which Briffon made his experiments, were taken from the king's collection.

The

The specific gravity of the blackish gray ambergris 7803 ; the weight of the cubic inch, 4 gros 3 grs. ; that of the cubic foot, 54 lbs. 9 oz. 7 gr. 35 grs.

### Chemical Properties.

It burns entirely away.

*Experiment I.* Ambergris burns, and is entirely dissipated, when placed on a red hot coal. It leaves behind an agreeable smell.

Melts with a less degree of heat,

If the combustion be conducted more slowly, in a crucible of platina, the ambergris melts, diffusing the same smell. The smell of a fatty substance may be distinguished likewise.

and is then a shining brown fluid.

Nothing remains in the crucible, but a greasy black spot.

50° of Reaumur's thermometer are sufficient to melt it, and a shining brown fluid is thus obtained.

Becomes volatile at 80° R.

At 80° it is volatilized in the form of a white vapour.

Its smell indicates an acid.

*Exp. II.* The smell perceived during its volatilization having led me to suspect the presence of an acid analogous to that of balsams, an experiment was made to ascertain this.

This acid detected by evaporating it under a bell,

A bit of ambergris was placed in a china capsule, covered with a bell, in which was suspended some litmus paper. This apparatus being placed on a sand-heat, the temperature was raised sufficiently to volatilize the ambergris, and the paper was very quickly reddened. Nothing now remained but to determine the nature of the acid ; and for this purpose Scheele's process for extracting the acid of Benjamin was adopted.

and proved to be the benzoic.

The product was examined, and left no doubt of their analogy.

On distillation,

*Exp. III.* The analysis by distillation in a retort added nothing to the knowledge we already possessed of the nature of ambergris.

it gives out a whitish acid liquor, with a light oil, and leaves a bulky coal.

A gentle heat melted it : on raising the fire it was decomposed, and there passed over into the receiver a whitish acid liquor with a white oil, partly soluble in alcohol, which gave it a yellow colour. In the retort remained a light and very bulky coal.

Imparts neither taste nor smell to cold water,

*Exp. IV.* Ambergris swims on water, and is not penetrated by it when cold. It imparts to it neither taste nor smell,

Boiling

Boiling water is equally incapable of altering its properties, and to boiling In this degree of heat the ambergris melts, and appears in the <sup>water it gives</sup> only a slight form of a brownish oily fluid; and a small quantity of black <sup>bitter taste.</sup> matter, insoluble in alcohol, separates from it. The filtered liquor has neither colour nor smell, it has however a slightly bitterish taste.

It is only in consequence of the temperature therefore that the ambergris melts, since on this being lowered it resumes the same properties as before.

*Exp. V.* Acids in general have little action upon ambergris. These agents likewise do not enable us to discover the constituent parts of this compound substance. <sup>Acids act but feebly on it.</sup>

Dilute sulphuric acid effects no change in it. The concentrated acid exposes a little oxide of carbon. <sup>Sulphuric.</sup>

The same phenomena are produced by the muriatic and oxygenated muriatic acid. <sup>Muriatic.</sup>

The nitric acid, at 18°, distilled over this substance in the pneumatico-chemical apparatus, produces nitrous gas, carbonic acid, and azote gas. <sup>Nitric produces nitrous gas, carbonic acid, and azote gas;</sup>

The azote gas arises no doubt from the decomposition of some animal matters, accidentally mixed with the ambergris, as may be observed in the examination of some pieces.

After the extraction of the elastic fluids, a thick liquor, inclining to a yellow colour, was found in the retort: this, on bringing it to a soft consistency, slightly swelled up; and being evaporated to dryness, in a porcelain capsule, what remained was a dry, bitter substance, of a golden yellow hue, shining and transparent, and exhibiting properties analogous to those of resins. <sup>and leaves a substance analogous to resins.</sup>

*Exp. VI.* Alcalis combine with ambergris, and form with it soluble soaps. <sup>Alcalis form soap with ambergris.</sup>

Into a crucible of platina were put one gramme, 592 (30 fr. grs.) of ambergris, with 531 thousandth of a gramme (10 grs.) of pure potash; it was gently heated; the mixture melted, without exhibiting any signs of the presence of ammonia; on cooling a homogeneous brownish mass was obtained.

On this were poured 30 grammes (one fr. ounce) of distilled water, which dissolved part of it. The solution was very alkaline.

The undissolved portion remained in a soft tenacious mass, which adhered to the fingers when warm.

A larger quantity of water was added, and the whole was dissolved.

**Caustic potash** does not facilitate its solution in cold water. **Ammonia** dissolves it with the aid of heat.

Caustic potash triturated for some time in a mortar with ambergris does not facilitate its solution in water.

Ammoniac does not act on ambergris cold, but when heated dissolves it; the mixture gradually becomes brown, and on evaporation yields a glutinous saponaceous substance, in all respects similar to that obtained by means of potash.

It is soluble in the fixed oils,

*Exp. VII.* The fixed oils, as those of rape, olive, &c. dissolve amber with the assistance of heat in a very short time; the solution is yellow and transparent, and becomes brown on being evaporated.

and in the volatile oils.

*Exp. VIII.* Volatile oils likewise dissolve ambergris.

Those of turpentine, savine, and hyssop, exhibit the same appearances. The solution assisted by heat takes place pretty readily.

On evaporating the solution a red magma is left, which is soluble in alcohol.

On evaporation a thick red magma is produced, incapable of complete desiccation, burning on the coals, and emitting a dense smok, of a smell resembling that of the ambergris. Alcohol dissolved this substance, and thence acquired a golden yellow colour, but it was precipitated from it by means of water.

Old volatile oils will not dissolve it.

If volatile oils be too old, they will not completely dissolve it, even with the help of long continued heat.

Soluble in ether.

*Exp. IX.* It dissolves very quickly in ether, even cold.

Alcohol separates its constituent parts.

*Exp. X.* The solution of ambergris by alcohol is the only one that is really capable of affording us any certain results. Its constituent parts may be separated by it in such a manner, that on reuniting them a compound is obtained, the qualities of which came very near those of the original substance.

Part dissolved in alcohol without heat;

3.821 grammes (one drachm) of ambergris were reduced to powder, put into a phial, and 61.143 grammes, (two ounces) of rectified alcohol were poured on them. A maceration of twenty four hours was sufficient to give the alcohol a deep yellow colour; it was filtered, and a fresh quantity of alcohol was poured on the undissolved portion. The solution was facilitated by increasing the temperature. The whole of the ambergris being dissolved, except a small quantity of black matter, the liquor was filtered while hot. It passed through the filter clear; but on cooling there separated from it a light pale yellow substance, part of which adhered to the sides of the vessel.

another part by means of heat; leaving a little black matter; and separating when cold.

The first solution in alcohol made without heat, and that which was poured off from the precipitate, were mixed together, and evaporated to the consistence of an extract: it was then of a reddish yellow colour, adhered to the fingers, had an agreeable smell, and a pleasant taste. The evaporation being continued to dryness, it appeared shining and transparent, grew soft between the fingers, and burnt in the same manner as resins.

This solution mixed and evaporated.

left a resinous substance.

The experiment was repeated, to determine the characters of these two substances more positively.

For this purpose ambergris was left to macerate in alcohol twenty-four hours as before; it was then filtered, and a fresh quantity of alcohol was added to the residuum, which was macerated in the same manner. The second liquor was less coloured than the first. A third portion of alcohol being poured on what was left undissolved; its colour was scarcely altered. The slight action of the alcohol on this residuum seemed to indicate, that it was no farther soluble in this menstrum; but I quickly found the contrary. I heated the mixture, and the whole was instantly dissolved, leaving about 212 thousandths of a gramine, (four grains) only of a black powder, which was nothing but oxide of carbone. The solution was filtered hot, and on cooling a whitish yellow glutinous substance was deposited, which was separated from the tincture.

The experiment repeated.

The black powder oxide of carbone.

This experiment shows us the possibility of separating by means of alcohol three very distinct substances; the first soluble in it cold; the second, by means of heat; and the third insoluble, which remains in the form of powder.

Thus three different substances separated.

To determine the characters of the first two substances, the tincture made without heat was first evaporated to dryness; when there remained in the capsule 1.167 grammes (22 grains) of a brown substance, dry and shining in its fracture, unalterable in the air, and growing soft with a gentle heat; 15° were sufficient to give it a tenacious and glutinous consistence; and being put on red-hot coals it was completely volatilized. If this experiment be made in a silver spoon, the volatilization takes place with the same rapidity, an odoriferous smell is diffused around, and no coaly residuum is left.

The first examined.

Suspecting this substance might be in some respect analogous to the resin obtained from propolis by Cit. Vauquelin, I instituted a comparison between them, and found the following differences:

Differs from the resin obtained from propolis



**Is in three re-  
spects.**

1st. It melts much more slowly; 2dly, it diffuses a dense odoriferous vapour, resembling a little the smell of honey; 3dly, it swells up, and leaves a very bulky coal.

**Is a true resin.**

Finally, this first substance obtained from ambergris, which may be considered as a true resin, is soluble in alcohol, and is precipitated by water. The solution reddens litmus paper, which proves too, that the alcohol dissolves the benzoic acid previously detected, either by burning the ambergris under a bell, or by treating it with lime.

**Examination of  
the second sub-  
stance.**

Nothing now remains, but to examine the product obtained by heated alcohol, after the resin is extracted by maceration.

I have said above, that there separated from the alcohol by refrigeration a substance, part of which subsided to the bottom of the vessel, and part adhered to the sides.

Being separated from the liquor, and properly dried, it remains a little bulky and light. Under the pressure of the finger it contracts and crumbles, but it is soon lengthened out and softened by the heat. It has a laminated texture, if it be suffered to cool slowly.

**Its properties  
the same with  
those of the  
adipoceros sub-  
stance found in  
the fatty matter  
of dead bodies.**

It retains between its particles a little water and alcohol, which may be separated by keeping it a short time in fusion. When melted over again it is much whiter than before, and no longer exhibits its former granulated texture. In fine, I have discerned in it all the properties of the adipoceros substance, discovered by Cit. Fourcroy in the fatty matter of dead bodies, and the properties of which he has described in a paper published in the 8th volume of the *Annals of Chemistry*.

From 3.821 grammes, (72 grains) of ambergris, 2.016 grammes, (38 grains) of adipoceros matter may be obtained.

#### *Recapitulation.*

**Recapitulation.**

From these experiments it appears we may conclude :

1st. That ambergris is a compound substance, which burns, and may be entirely volatilized.

2dly, that on distilling it alone we obtain from it a slightly acid liquor, and an oil partly soluble in alcohol, and of an empyreumatic smell.

3dly. That by sublimation, or by the process of Scheele, benzoic acid may be extracted from it.

4thly. That water does not act upon it.

5thly,

5thly. That by means of nitric acid a matter analogous to refins, mixed with the adipoceros substance, is extracted from it.

6thly. That the concentrated sulphuric, muriatic, and oxigenated muriatic acid, convert it to a coal, without dissolving it.

7thly. That with alcalis it forms a saponaceous compound.

8thly. That fixed oils, volatile oils, ether and alcohol, are the true solvents of ambergris.

9thly. And lastly, that alcohol affords the means of separating its constituent parts in the following proportions.

Adipoceros matter	-	-	-	-	2.016 grammes.	Its constituent Parts.
Refin	-	-	-	-	1.167	
Benzoic acid	-	-	-	-	0.425	
Coally matter	-	-	-	-	0.212	
					3.820.	

#### IV.

*An Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron, said to have fallen in India. By the Right Hon. CHARLES GREVILLE, F. R. S\*.*

THE experiments and observations made by Edward Howard, Esq. on certain stony and metalline substances said to have fallen on the earth, and the accurate description which the Count de Bournon has given of those substances, have, in my opinion, fully established the following fact, namely, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters.

That stony and metallic bodies have fallen on the earth is fully established.

The stones from Benares, that from Yorkshire, that from Sienna, and that from Bohemia, were the whole which had then been seen in England. They all contained pyrites of a peculiar character: they all had a coating of black oxide of iron: they all contained an alloy of iron and nickel; and the earths which served to them as a sort of connecting medium, corresponded in their nature, and nearly in their proportions.

Since the publication of Mr. Howard's and Count de Bournon's observations, I have received from France three new specimens from France.

\* From the Philos. Transf. 1803.

additional

Remarkable  
history.

additional specimens. Monsieur St. Amand very obligingly divided with me a specimen he had broken from a stone of about 15 inches diameter, preserved in the Museum of Bourdeaux, which stone fell near Roqueford, in the Landes, on the 20th August, 1789, during the explosion of a meteor; it broke through the roof of a cottage, and killed a herdsman and some cattle. M. St. Amand also gave me part of a stone he had preserved in his collection ever since the year 1790, when a shower of stones, weighing from  $\frac{1}{2}$  an ounce to 15 and 25 pounds each, fell in the parishes of Grange and Creon, and also in the parish of Juliac, in Armagnac; which fact was, at the time, verified by Duhy, Mayor of Armile, and published by Bertholon, in the *Journal des Sciences utiles de Montpellier*, in the year 1790.

The third specimen, I owe to the Marquis de Dree; it is a fragment, broken from a stone of 22 pounds weight, which fell near the village of Salles, not far from Villefranche in Burgundy, on the 12th of March, 1798; this was also accompanied by a meteor.

These three  
specimens agree  
in character with  
the others.

I content myself with the mere recital of the facts, in confirmation of the observations presented to the Society, as these three additional specimens have precisely the same characters, texture, and appearance, as the others in my collection; and are scarcely, by the eye, to be distinguished from them.

I should not, perhaps, have troubled the Society with this account, as my friend the Marquis de Dree, whose knowledge in mineralogy peculiarly qualifies him to investigate these subjects, has given me hopes of seeing his observations on them published; but a new evidence has lately fallen into my hands, and is the only one I have met with that ascertains the origin of native iron, which from analysis, had been suspected to have a common origin with the stones fallen on the earth. Con-  
versing with Colonel Kirkpatrick, whose researches have embraced both the literature and politics of India, and whose talents had placed him in very important situations in various parts of India, I inquired whether he had ever heard of any instances similar to the explosion of the meteor at Benares in 1798. He told me, he could not recollect having heard or read of any other instance, excepting one in the Memoirs written by the Emperor Jehangire, and of that he did not recollect the particulars. A few days after, having found the  
passage

Metallic stone  
that fell in India  
nearly two cen-  
turies ago.

passage in the original Persian, he was so obliging as to translate it. I consider it as an authentic fact; for the Emperor Jehangire was not a prince on whom his courtiers would idly venture to impose; and there can be little probability that an Aumil of a district should invent such a story, or be able to produce a substance apparently like iron, but which, on trial, differed from manufactured iron. Colonel Kirkpatrick's translation I have obtained his leave to communicate, with his attestation, to the Royal Society.

*Extract from the Memoirs of the Emperor Jehangire, written (in Persian) by himself, and translated by Colonel Kirkpatrick.*

A. H. 1030, or 16th year of the reign.—The following is among the extraordinary occurrences of this period.

Early on the 30th of Furverdeen, of the present year \*, and in the Eastern quarter, [of the heavens] there arose in one of the villages of the Purgunnah of Jalindher †, such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise, a luminous body [was observed] to fall from above on the earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time, the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched [by them] to Mahommed Syeed, the Aumil ‡ of the aforesaid Purgunnah, to advertise him of this event. The Aumil, instantly mounting, [his horse,] proceeded to the spot, [where the luminous body had fallen]. Here he perceived the earth, to the extent of ten or twelve guz §, in length and breadth, to be burnt to such a degree, that not the least trace of verdure, or a blade of grass remained; nor had the heat [which had been communicated to it] yet subsided entirely.

Narrative written by the Emperor Jehangire of a metallic stone that fell in the year 1620.

\* The first of Furverdeen of this year, (A. H. 1030,) corresponded with Saturday, the 27th of Rubbi ul Akhir; consequently, the 30th of Furverdeen fell on the 26th of Jummad ul Ouwl, or A. D. 1620.

† A purgunnah is a territorial division, of arbitrary extent. The purgunnah of *Jalindher* is situated in the Punjaub, and about 100 miles S. E. of Lahore.

‡ Aumil is a manager or fiscal superintendant of a district.

§ A guz is rather less than a yard.

Mahommed

Mahommed Syeed hereupon directed the afore said space of ground to be dug up; when, the deeper it was dug the greater was the heat of it found to be. At length, a lump of iron made its appearance, the heat of which was so violent, that one might have supposed it to have been taken from a furnace. After some time it became cold; when the Aumil conveyed it to his own habitation, from whence he afterwards dispatched it, in a sealed bag, to court.

It weighed upwards of four pounds, and was brittle.

Here I had [this substance] weighed in my presence. Its weight was one hundred and sixty tolahs.\* I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workman [soon] reported, that the substance was *not malleable, but shivered into pieces under the hammer.*†

Upon this, I ordered it to be mixed with other iron. Conformably to my orders, three parts of the *iron of lightning*‡ were mixed with one part of common iron; and from the mixture were made two sabres, one knife, and one dagger.

With the addition of one part of common iron to three parts of the metallic stone, excellent blades were made.

By the addition of the common iron, the [new] substance acquired a [fine] temper; the blade [fabricated from it] proving as elastic as the most genuine blades of Ulmanny,§ and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my presence, and found them cut excellently; as well [indeed] as the best genuine sabres. One of these sabres I named *Katai*, or the *cutter*; and the other *Burk-ferisht*, or the *lightning-natured*.

A poet|| composed and presented to me, on this occasion, the following tetra stich.

“ This earth has attained order and regularity through the  
 “ Emperor Jehangire :  
 “ In his time fell *raw* iron from lightning :  
 “ That iron was, by his world-subduing authority.  
 “ Converted into a dagger, a knife, and two sabres.”

\* A tolah is about 180 grains, Troy weight.

† Literally, “ it did not stand beneath the hammer, but fell to pieces.

‡ This expression is equivalent to our term *thunder-bolt*.

§ The name of the place here designed is doubtful.

|| The poet is named in the original; but the name is not perfectly legible.

The

The chronogram of this occurrence is contained in the words\* ( ) which signify "the flame of the imperial lightning;" and give the year (of the Hegera) 1030.

N. B. The foregoing translation (which is nearly literal) has been made from a manuscript that has been several years in my possession; and which, although without date, bears marks of having been written at a remote period.

WM. KIRKPATRICK.

## V.

*Analysis of the Natrolite.* By KLAPROTH.†

• THE fossil which forms the subject of this analysis, and to Natrolite found which I give the name of *natrolite*, for reasons to be stated at Högau, in hereafter, is found at Högau in Suabia, on the borders of Suabia. Switzerland. It is deposited in the crevices, or clefts and cavities of the sonorous porphyry. (Klingenstein Porphyry) from having a sound, nearly metallic, which form the mountains and rocks of Hohentwiel, Hohenkrähen, and Mägdeberg.

The colour of this fossil is a dirty ochraceous yellow, approaching sometimes to an Isabella yellow, at other times to a yellowish brown, intersected with concentric white lines. Its colour a dirty yellow.  
It is compact its internal fracture has a silky lustre. It breaks into wedge-like pieces, the edges of which possess little transparency; it is not very hard, extremely brittle, and of  $\approx 2,200$  specific gravity. Its properties.

## A.

a. 100 grains of natrolite, after having been ignited for some time in a silver crucible, lost nine grains. The figure of the stone was retained, but its compactness was considerably diminished. Ignited in a silver crucible.

b. Before the blow-pipe on charcoal, natrolite fuses quickly into a transparent glass, full of small air bubbles. Fuses quickly before the blow-pipe.

c. Natrolite placed in a clay crucible, and exposed to the heat of a porcelain furnace, fused into a transparent glass of a light brown colour. In a porcelain furnace melts into a brown glass,

\* The Persian characters are given in the Transactions. N.

† Abstract of an Essay in the memoirs of the Royal Academy of Sciences at Berlin, 1803, page 243.

d. In

with minute globules of iron in the surface.

*d.* In a charcoal crucible the mineral afforded the same product. The glass pearl exhibited on its surface minute globules of iron.

### B.

Dissolved in muriatic acid.

*a.* 100 grains of finely levigated natrolite, were mixed into a pasty fluid with water, put into a flask, and digested in moderately strong muriatic acid. The solution was soon effected, and it exhibited a reddish yellow gelatinous mass. After diluting it copiously with water, and continuing the digestion, the siliceous earth separated, which, being collected and dried, weighed 48 grains.

The siliceous earth being precipitated by water,

cubic crystals were obtained.

The dry mass digested in alcohol.

*b.* The fluid obtained in the last process on evaporation, yielded cubic crystals. The remaining fluid being further evaporated to dryness, the dry mass was pulverised, and digested with a gentle heat in alcohol. Having suffered the alcoholic solution to cool, a white saline powder was deposited; the alcohol was therefore decanted, and the powder collected, washed in spirit, and dried. The remaining alcoholic solution was afresh evaporated, a small quantity of the same saline powder became separated, which was added to that obtained before.

The residuum dissolved in water, and precipitated by ammonia.

*c.* The residue of the process *b* insoluble in alcohol, was dissolved in water. On adding to this solution liquid ammonia, a light flocculent precipitate became deposited. This being separated by the filter, the fluid which passed through, was evaporated by a gentle heat. The salt obtained, weighed when perfectly dry,  $31\frac{1}{4}$  grains.

The solution in alcohol decomposed by ammonia.

*d.* The alcoholic solution *b* (which from other experiments, was known already to contain nothing but alumine and oxide of iron) after being diluted with water, was decomposed by liquid ammonia, and the precipitate collected and dried. The fluid, from which this precipitate had been separated, was evaporated, and the mass strongly heated, so as to volatilize the muriate of ammonia that had been formed, when there remained two grains of salt, which being dissolved in water, yielded cubical crystals.

The precipitates digested in solution of potash.

*e.* The precipitate obtained by means of liquid ammonia *d*, together with that before produced *c*, were put into a solution of pot-ash, and digested with that fluid. A solution was effected, and oxide of iron separated, which, after being ignited, weighed  $1\frac{1}{2}$  grains.

4. The

# ANALYSIS OF THE NATROLITE.

f. The alkaline solution e was mingled with muriatic acid, till the precipitate produced, became redissolved, and was then decomposed by carbonate of soda. The precipitate obtained after being washed, dried, and ignited, weighed  $24\frac{1}{2}$  grains. It was alumine.

g. It remained still to examine the alkaline part of the fossil, which produced with muriatic acid the  $31\frac{1}{2}$  gr. c. and the two grains d. Taste, figure of crystals, and chemical reagents, proved it to be muriate of soda. A solution of it in water, mingled with a concentrated solution of tartarous acid, did not produce tartarite of potash. Another part of the solution, after being decomposed by sulphuric acid, yielded sulphate of soda.

Having ascertained by experiments that 100 parts of absolutely pure carbonate of soda \*, dried in a heat of ignition, when saturated with muriatic acid, lost 41 parts by weight of carbonic acid, and yielded  $120\frac{1}{2}$  parts of dry muriate of soda (the desiccation of which was not continued to decrepitation) we may conclude, that the above  $33\frac{1}{2}$  grains of muriate of soda contained  $16\frac{1}{2}$  of soda.

100 parts of the natrolite consequently yielded :

Siliceous earth B.	a	-	-	43. grs.
Alumine	f	-	-	24.25
Oxide of iron	e	-	-	1.75
Soda	g	-	-	16.50
Water	A.	a	-	9.—

Component  
parts of natrolite.

99.50

The small number of fossils which contain soda, is therefore augmented by one more. That soda was contained in this stone might perhaps have been expected, on account of its forming frequently the matrix of the sonorous porphyry, which,

\* In order to obtain perfectly pure carbonate of soda, I dissolve common carbonate of soda in water, and saturate this solution with nitric acid, taking care that the acid is a little in excess. I then separate the sulphuric acid by nitrate of barytes, and the muriatic acid by nitrate of silver. The fluid thus purified I evaporate to dryness, and fuse the nitrate of soda obtained, and decompose it by detonation with charcoal. I then eliminate the residue, filter, and crystallize the carbonate of soda.



it is now known, always contains this alcali, but as the quantity of soda contained in this fossil, is twice as large as that which exists in the sonorous porphyry, I have not hesitated at giving it the name of *natrolite*.

## VI.

*On the Employment of Aerostatic Machines in the Military Science, and for the Construction of Geographical Plans.\** By CITIZEN A. F. LOMET.†

Present state of  
aerostation.

Its improvement  
should be pro-  
moted by go-  
vernment.

Advantages to  
be derived from  
its use in war.

Instance.

THE aerostatic art is still in its infancy; and whatever progress may have been already made in it, it is impossible to foresee all the resources it may afford, or to determine the limits of its utility. Time and experience must fix our opinions respecting it; but it is of consequence to obtain the assistance of learned men and artists in this interesting pursuit; and as the smallest investigations of this nature are generally too expensive for individuals, it is necessary that the government should support an establishment particularly devoted to the practice and improvement of the processes which constitute it.

Aerostats will furnish, in presence of an enemy, one or more points of observation at pleasure, from which the positions he occupies may be reconnoitred, his movements studied, and his manœuvres judged of in the gross, or appreciated in the most minute detail. It may be presumed that these machines will become of the most indispensable utility in war, because they supply it with an extraordinary means, hitherto unknown, of making observations, which may in an instant determine the fate of battles, secure the dispositions for a vigorous defence, or at least point out the moment and the most convenient outlets for a retreat; and more particularly to draw attention to the advantages which an army may derive from balloons, it will be sufficient to remember the happy use made of them at the battle of Fleurus.

\* Adjutant-commandant, formerly keeper of the collection of models belonging to the Polytechnic School, and now at the head of the sixth division of the war department. (Military operations and movement of the troops.)

† From Journal de l'Ecole Polytechnique, Tome IV. p. 252.

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The Committee of Public Safety, and afterwards the Executive Directory, thought that the application of aerostats to military inquiries of every description ought to be studied and practised during peace. They were also desirous, that they might be employed in the construction of geographical plans, or at least in ascertaining the intermediate particulars of the territory between the points which had been geometrically determined. Having been charged with the experiments relative to these different applications, I purpose giving an account of the principal results.

The intention, from the first ascents, was to measure the angle formed by the visual rays falling on the eye of the aerial observer, from several determinate points on the earth. The unavoidable motion of the aerostat preventing the use of the graphometer in this operation, a recipiangle was at first substituted, suspended like a mariner's compass, by the assistance of which, it was hoped the measure of the angles would be easily taken, and particularly that they would be obtained with immediate relation to a horizontal plane. This attempt not having succeeded, it was necessary in future to make use of a sextant.

This instrument was every thing that could be desired for celerity, as well as for the facility and precision of the observations, but it has this inconvenience, in the case in hand, that it only shews the angle on a plane inclined to the horizon; and moreover, in its ordinary construction it furnishes no means of noticing this inclination. The perpetual agitation of the machine is another source of error; in fact, an aerostat, kept elevated and held by cords, is continually changing position; it moves in space, describing alternate ellipses, the curvature of which is modified to infinity, according to the violence of the wind, the elasticity of the cords, and the situation of the places to which it is fastened. It leaves then no trace of its variations, and does not permit the observer which it supports, to add to the measure of any angle whatsoever, that of the two angles necessary to connect the first with the plane of the horizon.

Nevertheless, for plans relative to the generality of military inquiries, and in all cases where a sketch of the figure of the earth is sufficient without attending to slight inaccuracies in distances, simple observations, made with the sextant, will answer the purpose, and furnish the means of operating with

Experiments undertaken by order of the French government.

First attempts at measuring the angle of the visual rays.

The use of the sextant is not free from error.

But is nevertheless applicable in many cases.

facility over a vast extent of territory, secure from the attempts of an enemy. But it is not equally serviceable in operations which require a rigorous exactness, and in which it is requisite to connect the angles with the centre of the station, and with the plan of the horizon.

Additional apparatus to render the sextant capable of indicating *all* the required angles.

The following is the mode in which I have endeavoured to fulfil the various objects:

The angles necessary for connecting the position of two objects with the centre of the station and the plane of the horizon, are, 1st. the angle comprehended between the rays falling on the eye of the observer from these points; 2d. the angles formed by each of these rays with the perpendicular.—We have seen, that it is impossible for the aerostatic observer to mark these three angles by taking them after each other; but if their measure were instantaneous, the difficulty would be overcome. This would therefore be the case if an instrument could be devised, which would give these three angles at once by a single observation; and as the sextant already shows the angle comprehended between the rays, the object in view is to add the necessary parts to that instrument for obtaining the other two at the same time.

Preliminary observations.

Let  $BAC$  (*Plate X.*) be the angle formed by the visual rays  $AB$ ,  $AC$ ; falling on the mirror  $A$  of the sextant from the objects  $B$  and  $C$ ; if the index  $AD$  be moved until the image of the object  $C$  reflected by the mirror  $A$  placed on the index coincides by double reflection on the mirror  $L$ , with the point where the object  $B$  is seen; and if they be both perceived at the same time by the observer looking through the telescope  $P$ , it is known, (*by the Theory and Use of the Sextant*) 1st, That the angle  $DAE$ , comprised between the index  $AD$  and the fixed radius or line of zero  $AE$  of the instrument, is always equal half the angle  $BAC$ , the measure of which is required: 2d, That the line  $RB$ , which is supposed to pass through the axis of the telescope and the centre of the mirror  $L$ , is always directed to the point  $B$ , and is usually taken for the side  $AB$ ; the error arising from the small distance  $AR$  being considered as nothing in practice: hence, if we suppose a visual ray passing from the point  $R$  to the object  $C$ , the angles  $BR C$  and  $BAC$  may be reputed equal, and be taken indiscriminately for each other.

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This being premised, if a ruler be placed in the direction R B, it may be considered as in that of the side A B, and if we can succeed in fixing a second ruler in such a manner that the moveable index shall carry it into the direction R C, at the instant that the images of the two objects B and C are brought into one at the point L, it is evident that these two rulers will form between them the angle B R C, and consequently the angle B A C.

Method of ascertaining the angle formed by visual rays.

To accomplish this, let us suppose a sort of false square, S R Q, situate in the plane of the instrument, and moveable at its axis on a pivot fixed at the point R, at the intersection of the lines A R and R L; making the angle S R Q, comprised between its arms, equal to the angle E R L, and the side R S equal to the distance A R. If now we suppose that the extremity S of the side R S, is retained by a button in a groove M N, worked in the moveable index, the movement of it will be communicated to the false square in such a manner that the angle L R Q will always be equal to the angle B A C, and consequently the side R Q will be placed in the requisite direction.

In fact, the triangle A R S being isosceles in its form, the exterior angle S R E = R A S + A S R. = 2 R A S = B A C; but S R Q being equal to E R L, if the common angle S R L be taken away, there will remain the angle S R E = L R Q = B A C.

Now let us fix under each of the two rulers R L and Q R a small graduated quadrant, suspended in such a manner that it will place itself in the vertical plane of the side corresponding to the angle observed; let us affix to each of these quadrants a plummet, composed of a stiff arm moveable upon a pivot, and furnished with a nonius index and a weight, which gives it a constant tendency to assume a vertical position, in whatsoever situation the sextant may be placed; finally, let the whole be so disposed that the index of each plummet may be retained at will, at the division indicated on the limb by the effect of the suspension, and this by means of a trigger, which can be pulled at the exact instant of observing the principal angle in the points of reflexion. It is evident that the simultaneous action of the two rulers and the plummets will show the three angles sought, and that nothing remains but to reduce, by calculation, the angle B A C to the plane of the horizon.

Observation of the angles formed by the visual rays and the perpendicular.

From which those on the plane on the horizon are obtained by calculation.

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**Success of the instrument.**

This instrument, arranged in the manner here described, produced every desired effect in our experiments. The invention, as simple as it is happy, may become very important from the useful applications of which it is susceptible; and there is already reason to hope that, by bringing this to perfection, or by the formation of some analogous instrument hereafter, there will be a possibility of executing trigonometric operations with much correctness, by the assistance of aerostatic machines, notwithstanding their continual motion.

**Probable future advantages.**

**Method of connecting all the observed angles in one common centre.**

It was not enough to have discovered the means of connecting the angles with the plane of the horizon: it was still desirable, that all the angles observed during ascents in any one place, should have a relation to the common centre of observation. To accomplish this, it was necessary to keep a register, by some means, of the situation of the machine at the precise moment of measuring each of these angles. This was done by dropping from the aerostat, at that instant, a small stake, leaded and furnished with an iron point. This stake fell rapidly to the earth, into which it stuck, and marked a point corresponding to the summit of the angle measured. It was then easy to compare the position of this point, with that of one taken for the common centre of the observation, and thence to deduce the necessary corrections. It must however be noticed, that the stake, when abandoned to itself, acquires, at the instant of its fall, a compound motion which partakes of that of the aerostat, and consequently is not exactly vertical; but the error which results from this deviation is but slightly perceptible in practice.

**Application of the process.**

The calculations and ordinary processes of descriptive geometry will furnish all the means of making use of these different observations, and of expressing the results on paper; not only for their application to the construction of maps, but also to ascertain heights compared with the level: but we shall not in this place enter into any details on that head.

**Inaccuracy to be expected from an inexperienced observer.**

The observer engaged making these first experiments, soon perceives that the involuntary embarrassment, occasioned by the novelty of his situation, when he finds himself insulated and suspended at a height of seven or eight hundred metres, has a considerable influence both on the fidelity of his observations and on the time necessary for making them. All certainty depends, in fact, upon the confidence and readiness

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of the observer; and it cannot be concealed, that it may produce great inconveniences, because this difficulty of operating opens wide limits for the errors which it is possible to commit.

From this last observation it will be seen: 1st. That it is indispen- Conditions necessary to make correct obser- sably necessary to have acquired a great aptitude for these sort of observations, to be able to execute them with precision: vations.

2d. That the processes just described are more satisfactory in theory than they would be in those applications which require strict accuracy; and, that though there are situations in which nothing can be substituted for their use in the construction of some figured plans, it is at least proper never to use them in preference to those means of observation which are better known, and which can be employed with more certainty.

But it cannot be too often repeated, at the same time, that aerostats furnish the means of giving the most lively interest to the delineation of the figure of the earth, in maps of all descriptions; and that their use is of the greatest assistance in the formation of plans, the outline of which may be sufficiently defined by a simple eye-sketch. The aerial observer, by discovering a vast extent of country, accustoms himself fully to consider the general organisation of the asperities of the country, and even the particulars of its varieties, as well as the tone of colour, which appear to give a different character to each portion of territory. If this new method of observation be cultivated with assiduity, it will doubtless lead to a sensible improvement in the art of drawing plans. But to benefit by the advantages which it affords, it is necessary, that those who are destined to this employment should join a profound knowledge of geometry to a great facility in designing landscapes. May they be convinced of the importance of this truth, and assure themselves that no part of the plan can arrive at perfection, either ornamental or relative to civil and military purposes, unless strict accuracy in the outline is accompanied by that fidelity of expression which is capable of producing in those who inspect the plans, all the ideas which the observer had formed from the aspect of the country. Advantages to be derived by employing balloons in surveying countries.

From all that has been said, we may conclude, that the aerostatic art combines properties no less valuable than unquestionable in topographic operations and military researches; that its per- Valuable properties possessed by these machines. fection

Military use of them.

fection may produce new and invaluable properties; and that it would be equally impolitic to neglect the use of these machines, or not to obtain for them the information to be derived from reflection and experience. We shall terminate this memoir by an observation relative to their military uses. Our enemies would not fail to oppose to the creative industry of France, an industry of imitation: they would also have their balloons and ballooners (*aerostiers*.) The influence of this innovation in war is of a nature to spread with rapidity, and it must soon cease to favour any nation exclusively. But even in this case the art of aerostatic machines will have acquired a higher degree of interest, because another element shall then be in the power of man, in which the efforts of genius and industry may be substituted instead of the inconsiderate devastations of force; and this observation ought to interest the friends of humanity in bringing them to perfection.

## VII.

### *Chemical Analysis and Properties of Arseniated Hydrogen Gas* By PROFESSOR TROMSDØRF.\*

Scheele discovered an inflammable arseniated gas.

Its properties.

THE immortal Scheele, in his essay on arsenic and arsenic acid,† mentions an inflammable arseniated gaseous fluid, of which he says: "*Hinc intelligas, hunc aerem inflammabilem esse, regulumque arsenici solutum tenere.*" Scheele states, that he obtained this gas during the solution of tin in arsenic acid. The properties of this gas, as pointed out by him, are the following. Arseniated hydrogen gas is insoluble in water; it does not render lime-water turbid; mingled with atmospheric air, no diminution of bulk ensues; on bringing the flame of a taper in contact with this mixture, a loud detonation follows, and metallic arsenic is deposited. Interesting as the observations here pointed out must appear to every chemist, the object has been neglected by succeeding operators.

\* From a memoir, read in the Royal Academy of Sciences at Berlin, 1803, p. 370.

† C. H. Scheele Om Arsenick och dess Syra; Kongl. Svensk. Vetenskaps Academiens *Handlingar*. Ar. 1775. V. xxxvi. 263.

Proust is the only philosopher who mentions this gas : \* he Proust mentions obtained it by digesting arsenious acid and zinc, in dilute sulphuric acid ; on burning the gas, he obtained sometimes arsenious, at others arsenic acid. Being persuaded that the formation and properties of this gas deserved a closer examination, I instituted a series of experiments, the results of which are as follows.

*Methods of obtaining arseniated Hydrogen Gas.*

1. There are a variety of processes for obtaining arseniated hydrogen. It is produced by heating tin filings in liquid arsenic acid. This method is the most expensive and most tedious. Processes for obtaining arseniated hydrogen gas.

During the evolution of the gas in this process, arsenic, alloyed with tin, is precipitated, and the fluid obtained, holds in solution, arseniate of tin.

2. It is likewise formed by treating in a similar manner, arsenic and iron with muriatic acid.

3. Arseniated hydrogen is also produced by heating a mixture of arsenious acid, iron filings and muriatic acid. The fluid, in this case, contains muriate of iron and muriate of arsenic.

4. Tin filings and arsenic acid yield this gas under similar circumstances.

5. Four parts of granulated zinc and one of arsenic, treated in a similar manner with sulphuric acid, previously diluted with two parts of water, afford arseniated hydrogen very readily.

The gas obtained according to either of these processes, is Best produced from zinc, arsenic, and sulphuric acid. nearly alike, but that produced according to the last process seems to be the most perfect gas, for it contains no excess of sulphuric acid. When arseniated hydrogen is produced by means of zinc, arsenic, and dilute of sulphuric acid, the quantity of arseniated hydrogen is less than the quantity of hydrogen which would be obtained in decomposing water in a similar manner, without the interposition of arsenic. The residue, after the evolution of the gas has ceased, contains metallic arsenic; part of the hydrogen must therefore have acted on the oxygen of the arsenic acid, in order to reduce it to the metallic state. From what has been stated, it appears that arseniated hydrogen contains arsenic in a metallic state, and not in the state of arsenious, or arsenic acid. This will become more obvious in the sequel of this paper.

\* Journ. de Phys. T. II. p. 173.



*Physical Properties of Arseniated Hydrogen Gas.*

Its physical properties.

Arseniated hydrogen is a permanent elastic aeriform invisible fluid. It is a true chemical compound. Proust asserts that it deposits arsenic: This however I have never been able to observe, if the gas were *pure*. It has an alliacious fetid smell. It extinguishes burning bodies. It is not absorbable by water; but when this fluid is freed from atmospheric air, it takes up a small quantity of the gas which becomes disengaged again by mere agitation. It does not change the colour of tincture of litmus. The specific gravity of arseniated hydrogen is, at 28' barometrical pressure = 0,5293, or, one cubic inch (old French measure) weighs 0,2435 grains. It is therefore lighter than oxygen, nitrogen, atmospheric air, carbonic acid, nitrous gas, ammonia, and gaseous oxide of carbon, but heavier than hydrogen and sulphurated hydrogen gases. It is absolutely fatal to animal life.

*Chemical Properties of Arseniated Hydrogen Gas.*

Its chemical properties.

Mixed with nitrous gas.

Arseniated hydrogen, mingled with atmospheric air, suffers no chemical change, but mere dilution. The same holds good with respect to nitrogen. When mingled with nitrous gas, a diminution of 0,02, or 0,03 takes place, which sometimes even amounts to 0,05. To ascertain the nature of this gas, I mixed two parts of arseniated hydrogen, with one of nitrous gas, and gradually added oxygen, till no further diminution of bulk ensued. On presenting to this mixture a lighted taper, a loud explosion took place, accompanied with flame. Probably part of the oxygen added, remained uncombined; for a mixture of two parts of nitrous gas, and three of arseniated hydrogen, could not be inflamed by the taper; arseniated hydrogen is miscible with hydrogen, with carbonic acid, and with ammonia in all proportions.

With oxiginized muriatic acid gas.

Into a cylinder half filled with arsenic and hydrogen, I sent up bubbles of oxiginized muriatic acid gas. The bulk of the gas was diminished, heat was evolved, and metallic arsenic was deposited in a crystalline state. On adding to the mixture an additional dose of oxiginized muriatic acid gas, white fumes appeared, and the deposited metal vanished. The same experiment was repeated successively, taking care to add no more of the latter gas, than was just sufficient to occasion the precipitation

precipitation of metallic arsenic. The collected metal yielded nitrous gas, by the affusion of nitric acid, and on adding to this mixture muriatic acid, arsenic acid was produced. The arsenic deposited in the manner stated before, when laid on ignited coals, became volatilized in thick white fumes, yielding arsenious acid. The precipitation of metallic arsenic must be ascribed to the decomposition of the oxygenized muriatic acid gas; the oxygen of this gas uniting with part of the hydrogen of the arseniated hydrogen, and forming water, and thus separating the arsenic. For the arsenic is capable of being oxidized by the muriatic acid. Should it be imagined, that arsenic existed in arseniated hydrogen, in the oxidized state, and that it became precipitated by the oxygenized muriatic acid robbing it of its oxygen, we suppose things analogically erroneous, for the oxygenized muriatic acid is more capable of giving out oxygen than of taking it. The experiments of Chenevix seem perhaps hostile to this assertion; but the experiments of this philosopher merely prove that the oxygenized muriatic acid is capable of combining with an additional dose of oxygen, and constituting with it a hyperoxygenized muriatic acid. This certainly cannot be the case in the present instance, as will appear more evident from what I shall state presently.

I filled a cylinder in the mercurial pneumatic trough, with arseniated hydrogen, and sent up into it as expeditiously as possible, a quantity of oxygenized muriatic acid gas. The result was evolution of heat, diminution of volume, and the inner sides of the cylinder became covered with a kind of dew. A formation of water had therefore actually taken place in this experiment. Into another dry cylinder half filled over mercury, with arseniated hydrogen, I introduced dry muriatic acid gas. In this case no diminution of bulk, no separation of arsenic ensued; no change at all took place. Repeating the same experiment, I introduced into the cylinder a small quantity of water; the muriatic acid gas was absorbed, and the residue was arseniated hydrogen unaltered.

Into a cylinder half filled with oxygenized muriatic acid gas, I passed gradually arseniated hydrogen in small bubbles at a time; in this case no metallic arsenic was separated, but thick white clouds appeared. On continuing the addition of arseniated hydrogen till no more white fumes appeared, metallic arsenic was deposited. It follows from this experiment, that  
when

Further experiments with this gas over quicksilver.

when a *small* quantity of arseniated hydrogen is made to act upon a *large* quantity of oxiginized muriatic acid gas, part of the oxigen of the oxiginized muriatic acid gas combines, not only with the hidrogen of the arseniated gas, and forms water, but the metallic arsenic also becomes oxidised. Reasoning from this fact, we should be inclined to believe, that a mutual decomposition of both the gases could be thus effected; but this cannot be accomplished; a diminution of bulk indeed takes place to a certain extent, but the complete disappearance of both the gases cannot be effected. If the admixture of arseniated hydrogen, with this oxiginized muriatic acid gas, be continued no longer than white clouds appear, and the residue be then examined, it will be found to consist of hydrogen and oxiginized muriatic acid gases; and the mixture detonates at the approach of a taper. The oxiginized muriatic acid gas can only be separated with difficulty by long agitation, in contact with water, and it seems as if it were become less soluble in that fluid. If the separation of this gas be accomplished, the remaining arseniated hydrogen burns with a pure flame, void of alliaceous odour, and contains no vestige of arsenic, as shall be proved hereafter. From what has been stated, the following theory may be formed.

Theory of the decomposition of arseniated hydrogen gas.

Arsenic, in combination with a certain portion of hydrogen, constitutes arseniated hydrogen gas. On presenting to this combination oxiginized muriatic acid gas, the oxigen of this gas combines with the hydrogen, which held in solution the arsenic, and the latter is separated. If more oxiginized muriatic acid be added than is necessary for this purpose, the portion of oxiginized muriatic acid gas does not act further upon the hydrogen, but merely upon the arsenic, and the latter becomes oxidised.

Arseniated hydrogen gas mixed with hydrogen.

Hydrogen and arseniated hydrogen may be mingled without decomposing each other; the decomposition can only be effected by the contact of fire; but if we mingle hydrogen, holding in solution sulphur and oxiginized muriatic acid gas, the decomposition and formation of water is instantly effected. This is likewise the case with arseniated hydrogen gas.

Hydrogen combined with metallic substances.

Hitherto no combination of hydrogen with a metallic substance has been known; but it is highly probable, that such combinations may exist. This indeed seems to be the case in the formation of this gas on which we are treating. If this be admitted,

mitted, a division of hydrogen must take place, in the following manner; one part of it must unite with the oxygen of the oxygenized muriatic acid gas, to produce water; another part must fall down with the arsenic; and another portion remains combined with caloric, in the form of hydrogen gas.

Hydrothian acid gas \* and arseniated hydrogen do not act upon each other. To a mixture of equal parts of hydrothian acid gas, and arseniated hydrogen gas, I added gradually oxygenized muriatic acid gas; a diminution of volume instantly took place, accompanied with liberation of heat, and a deposition of yellow sulphurized arsenic (orpiment). On adding an additional quantity of gas, the precipitate acquired a beautiful orange red colour, and on continuing the addition of oxygenized muriatic acid gas, white clouds were produced, the precipitate detached itself from the sides of the vessel, and were gradually converted into a pulverulent substance of a yellowish white colour.

The results of these experiments are obvious, and might have been expected *a priori*. But they may serve as a test to discover the presence of arseniated hydrogen, when contained in other gases.

I mingled one cubic inch of arseniated hydrogen with ten of nitrogen, and one of hydrothian acid (sulphurated hydrogen gas;) on adding to this mixture a small quantity of oxygenized muriatic acid gas, yellow sulphurized arsenic was instantly deposited. It is not improbable, that arsenic is likewise soluble in other gases, and in this case the hydrothian acid (liquid sulphurated hydrogen,) in conjunction with oxygenized muriatic acid, would prove a useful re-agent for discovering the presence of it.

A lighted taper immersed in a vial filled with arseniated hydrogen, is instantly extinguished; at the same time that the gas burns at the orifice of the vial with a lambent white flame, diffusing a disagreeable odour, and much white fumes, which are arsenious acid. If the gas be inflamed in a phial with a small orifice, the flame descends gradually down to the bottom of the phial, which becomes lined with a coat of crystallized metallic arsenic. In this case therefore the hydrogen alone burns.

\* The name given by the Germans to sulphurated hydrogen gas, on account of its possessing the properties of an acid.

Explodes with oxygen.

If two parts of arseniated hydrogen be mingled with three of oxygen, and a taper be presented to the mixture, an explosion takes place; no metallic arsenic is separated, but the products are arsenious acid and water: soap-bubbles with the mixture of these gases, explode with a bluish white flame, leaving a white smoke and strong alliaceous odour. Equal parts of arseniated hydrogen and oxygen gases, fired in like manner, do not explode so loudly, but the report is accompanied with a much more vivid flame. A stream of arseniated hydrogen, issuing from a bladder fitted with a stop-cock, and set to burn in a large receiver filled with oxygen, yielded arsenic acid. The combustion in this manner is uncommonly beautiful; the gas burns with a blue flame of uncommon splendor:

Burns beautifully in oxygen gas.

Composed of metallic arsenic and hydrogen.

Two parts of arseniated hydrogen, and one of oxygen gas, being detonated in a close vessel by means of the electric spark, left a small residuum; on repeating the experiment, the detonating tube broke during the explosion, which prevented the examination of the residue. From what has been so far related, it becomes evident that the constituent parts of arseniated hydrogen gas, are metallic arsenic and hydrogen. Were it possible to determine with absolute certainty, that no increase of volume took place during the solution of arsenic in hydrogen, the proportion of the constituent parts of this gas might be ascertained thus:

French weight and measure.

One cubic inch of hydrogen, weighs - 0,0353

One cubic inch of arseniated hydrogen, weighs 0,2435

Deducting the former from the latter, we get 0,2082

Which is the quantity of arsenic dissolved in the gas, consequently one cubic inch of arseniated hydrogen gas, consists of 0,0363 hydrogen, and 0,2082 arsenic; and one cubic inch of this gas contains about  $\frac{1}{4}$  grain of metallic arsenic.

#### *Habitudes of arseniated Hydrogen to Acids.*

Its habitudes to acids.  
Concentrated nitric acid.

Into a phial, containing about eight cubic inches of arseniated hydrogen, I poured a half cubic inch of concentrated nitric acid. The moment the acid came into contact with the gas

gas. The phial was filled with dense red fumes, a white flame pervaded the vessel, and a detonation ensued.

On repeating the experiment with dilute nitric acid, no ignition took place. The residuary gas was pure hydrogen, and the water contained arsenic acid. Fuming concentrated nitrous acid therefore is capable of oxidising the arsenic contained in this gas, at the same time that the oxygen of the acid burns with the hydrogen of the gas, and produces water; whereas weak nitric acid is only capable of oxidizing the arsenic, without acting upon the hydrogen present.

Into a glass tube, furnished with a stopper at one extremity, and closed at the other, I introduced eight cubic inches of arseniated hydrogen, to which were added two cubic inches of nitro-muriatic acid. After having agitated the fluids, on opening the tube under water, a diminution of one cubic inch took place. The residuary gas was pure hydrogen. It is remarkable that, during the addition of the nitro-muriatic acid, a black powder separated, which again disappeared on agitating the tube. Nitro-muriatic acid acts therefore in the same manner upon this gas, as oxygenized muriatic acid gas. It effects first a separation of the metallic arsenic, and then oxygenizes this metal. Liquid oxygenized muriatic acid decomposes arseniated hydrogen by mere agitation; the residue is hydrogen. Muriatic acid exercises very little action upon arseniated hydrogen; but merely dissolves a minute portion of it, which may be expelled again by heat. Concentrated acetic acid has no effect upon it.

Into a glass cylinder holding eight cubic inches of arseniated hydrogen, I poured one cubic inch of concentrated sulphuric acid, and then closed the tube. At the moment of the addition of the acid, the cylinder became lined with a coat of bright metallic arsenic, so as to resemble a looking-glass. On agitating the cylinder, the coating resolved itself into a brownish black powder, which, after a few days, assumed the colour of Kermes mineral. On opening the cylinder under water, a diminution of bulk ensued, and the residuary gas proved to be hydrogen. The experiment was repeated, and yielded the same results. The sulphuric acid employed in this experiment, had acquired a penetrating pungent smell, and was examined, after having been neutralized by ammonia, in the following manner:

Ammoniate of copper, on being mingled with it, acquired a greenish colour. Hydrosulphuret of ammonia instantly occasioned a copious yellow precipitate.

Water impregnated with sulphurated hydrogen gas, occasioned a similar effect. From the results of these tests it becomes obvious, that the acid consisted of sulphuric, sulphureous and arsenic acid. In order to be certain in this respect, I mingled a few drops of liquid arsenic acid with a mixture of sulphuric and sulphureous acid, neutralized the fluid with ammonia, and submitted it to the same tests. The results of this mixture were analogous to the former. The decomposition of the arseniated hydrogen gas, is probably analogous to the decomposition of this gas, by means of oxygenized muriatic acid gas. The sulphuric acid first gives up part of its oxygen to the hydrogen of the arseniated gas, and occasions the separation of the arsenic; which, at the expense of the remaining portion of oxygen of the sulphuric acid, becomes afterwards oxygenized, and constitutes the arsenic acid.

#### Theory.

Its habitudes  
to metallic  
solutions.  
Ammoniate of  
copper.

#### *Habitudes of arseniated Hydrogen Gas to Metallic Solutions.*

I caused a current of arseniated hydrogen gas, to pass through a solution of ammoniate of copper. A metallic pellicle appeared on the surface of the fluid, which suffered no other change.

Muriate of tin.

Into a bottle filled with arseniated hydrogen gas, I dropped a solution of muriate of tin. On agitating the solution, it acquired a brown colour, a partial diminution of the gas ensued, but the solution of tin was not converted into an oxidized muriate of tin, which would have been the case, if the arsenic existed in the gas in an oxidized state.

Nitrate of lead.

Nitrate of lead, on being brought into contact with arseniated hydrogen gas, became turbid, and deposited a precipitate, which was arseniate of lead.

Nitrate of silver.

Nitrate of silver submitted to the action of the gas, became instantly of an intense black, and a pellicle of metallic silver collected on the surface of the fluid. The residue of the gas, which had been made to act on the oxide of silver for some time, had all the properties of pure hydrogen.

A good test.

This experiment shows, that nitrate of silver might be employed for detecting the presence of arseniated hydrogen; for

as long as a minute quantity of arsenic was present, a black precipitate ensued, whereas *pure* hydrogen has no effect upon this re-agent.

I passed into a concentrated solution of nitrate of silver, a stream of arseniated hydrogen, collected the black metallic precipitate, washed and dried it. The fluid obtained in this process did not disturb the transparency, or change the colour of ammoniate of copper. Neither liquid sulphurated hydrogen, tincture of galls, nor potash, had any effect upon it. It contained therefore neither silver nor arsenic. The precipitate before obtained, acquired a metallic lustre on being saturated; laid on ignited coals, it diffused an odour of arsenic, and it yielded by fusion a button of silver. It was an arseniate of silver.

Arseniated hydrogen passed into a solution of nitro-muriate of gold, occasioned a precipitate; on the surface of the fluid appeared a pellicle of metallic gold; and the sides of the vessel, in contact with the fluid, became beautifully gilded. The fluid through which the gas had been passed, examined in the usual manner, proved to contain no vestige either of gold or arsenic. The precipitate greatly resembled charcoal dust, interspersed with minute particles of gold.

It is highly probable, that arseniated hydrogen is capable of decomposing all metallic solutions, the basis of which is either nitric, or muriatic acid, and probably other acids.

#### *Habitudes of arseniated Hydrogen Gas to various other Bodies.*

Expressed oils, on being agitated for some time in contact with arseniated hydrogen, absorbed part of the gas, and acquired a deeper colour.

Alcohol suffers no change from arseniated hydrogen. Solution of potash, and liquid ammonia, do not absorb it.

Such are the properties of this gas, the investigation of which I shall continue as soon as my health is restored, it being so considerably injured by the unavoidable inhalation of this gas during the course of these experiments, which gives me ample reason to conclude, that the gas must be highly poisonous.

D. J. B. TROMSDORFF.

*Enfurth, Feb. 1803.*



## VIII.

*Account of an Eudiometric Apparatus, contrived and used by Dr. HOPE, Professor of Chemistry in the University of Edinburgh\*.*

Dr. Hope's eudiometric apparatus.

SINCE the discovery of the uncertainty with which the application of nitrous gas to atmospheric air, and other mixtures, containing oxygen is attended, it has been found desirable to present solid or liquid substances for the absorption of that principle. This, on first consideration, may seem at least as easy to be done, as to mix two gases; but it is by no means so, because the liquids in particular possess a degree of chemical activity, which renders it inconvenient to immerse the hands in them, or to expose their surfaces to the open air, especially when it is attempted to accelerate their operation by means of agitation, so as to obviate the principal objection to their use, the tardiness of the process.

The apparatus of Dr. Hope, which he uses in his lectures and in his experiments, is at once simple and effectual, and I have the pleasure of inserting the following correct description with his permission.

The apparatus consists of two bottles, which are represented in *Plate XII*, connected together in the manner in which they are used; A represents a small bottle which may be nearly two inches in external diameter, and three in length, having a neck and stopper at D, and another neck as usual at C. It is destined to contain the eudiometric liquor. B represents a larger bottle, which may be nearly of the same diameter, or rather of somewhat less, but  $8\frac{1}{2}$  inches long. The neck of B is fitted accurately by grinding into the neck of A at C.

The method of using this apparatus is very simple: introduce in the ordinary way into the bottle B, the air or gas

\* As the description at p. 61, of the present volume is in some respects inaccurate through haste, and the figure, being an outline, appears as if the neck of the upper vessel protruded so far into the lower, as to prevent the ascent of a portion of the gas after agitation,—I have chosen rather to give an entire description and drawing in this place, than adopt the less acceptable process of amending and correcting.

till it is full ; then fill A with the absorbing liquor, for exam-  
 ple with a solution of sulphuret of lime, which Dr. Hope  
 commonly employs, and covering the mouth with a flat piece  
 of glass, plunge it under the surface of water, and there in-  
 sert the neck of B.

Dr. Hope's eu-  
 diometric appa-  
 ratus.

The compound vessel is then removed from the water, and inclined till a sufficient quantity of the liquor flows into B. It is now well shaken, and the agitation ought to be continued till the absorption is completed—Left the diminution of the density of the included elastic fluid should retard the absorption of the oxygenous portion ; from time to time the apparatus, in the position in which it is represented in the figure, is to be placed in a plate full of water, and the stopper D is to be loosened, or so far withdrawn, as to allow this fluid to enter to fill the place of the absorbed gas.—By this admixture of water the liquor is diluted, but not to such a degree as in any measure to interrupt the advancement of the process, unless indeed when the gas abounds very much in oxygen.

When a gas of this description is the subject of experiment, it may be proper to use an apparatus, of which the bottle A is made of greater capacity in relation to the size of B, than in the proportion already assigned.

As soon as it is observed, that after reiterated agitation, and opening the stopper D, the liquor does not rise higher, the absorption may be considered as completed, and the operation may be finished by allowing the instrument to regain its original temperature, in case, from want of due precaution, it may have been affected in this respect by the warmth of the hand in the course of the experiments.

If the bottle B be graduated, the amount of the absorption may be determined at once, by plunging the apparatus into water to the level of the included liquid, and removing the stopper, otherwise the residual gas may be transferred into a tube, expressly graduated for measuring gases.

By this convenient contrivance, we see that the liquid is economized and the celerity, neatness, and precision of experiment are ensured. The size here mentioned is very well adapted to the purposes of public exhibition, but it is almost needless to remark, that it may, and in general ought to be made considerably smaller for the ordinary eudiometric experiments.

Dr. Hope suggested that the apparatus might be made still more simple without impairing its merits, in any considerable degree, particularly when small volumes of gas are to be examined. This is done by using a small bottle having one neck only, and having a graduated tube nine or ten inches long, and from half to three quarters of an inch in diameter, accurately adjusted to fit into it, but not projecting into its cavity.

If the bottle have twice or thrice the capacity of the tube, the same solution of sulphuret of lime may be repeatedly used, and the absorption will be more expeditious.

In employing this instrument, the manipulation is in all respects the same as above described, excepting when the progress or termination of the operation is to be discovered. For this purpose, loosen the connection between the tube and the bottle, in a degree sufficient to allow the ingress of the water of the trough, in which the eudiometer must then be immersed.

This apparatus equally unites dispatch, œconomy of eudiometric liquor, and convenience of management.

## IX.

*Description of an Apparatus for drying the Products of Chemical Analysis which is also useful for Experiments of Congelation.*  
By Mr. FREDERICK ACCUM. Communicated by the Inventor.

Apparatus for drying precipitates on the water bath :

THIS apparatus, Plate X. is extremely useful in drying such products as absolutely require a temperature not exceeding  $212^{\circ}$ ; such as fulminating mercury, Chenevix's fulminating silver, and other explosive compounds. The substance to be dried must be placed in the conical glass vessel B, and when the vessel E is filled with water up to the side tube D; the desiccation may be performed without any risk of explosion, or any further trouble, by putting the apparatus over a lamp, and keeping the water in a state of ebullition. (*See the lower drawing in perspective.*) I have found it particularly useful in the desiccation of the precipitates obtained in the analysis of minerals. It is well known that the same mineral, analysed

analysed by different chemists, has been found to yield different proportions of the same ingredients, and that the difference of proportions of the constituent parts, in many cases, is often more apparent, than real; arising entirely from the various degrees of desiccation that has been employed by different analysts, and sometimes even by the same person. This point is of such importance, and is productive of so much trouble, that every chemist who has analyzed a mineral water, or crystallized and separated small quantities of deliquescent salts, will at once perceive the utility of the apparatus in this respect.

This apparatus may likewise be used as a *water-bath*. In the same used that case, the conical glass vessel B is removed, and the inner as a water bath: tin vessel E filled with water; into this, retorts, flasks, gallipots, vials, bottles, &c. may be immersed for promoting the processes of distillation, digestion, solution, evaporation, &c. or it may be used as a *sand bath*, (it being hard soldered) by or sand bath. filling the tin vessel with sifted sand, for performing those operations which require a higher temperature, than that of boiling water.

When the instrument is required to be used as a *freezing Method of freezing apparatus*, the bottom cover G is to be taken off, and the cavity between the interior, and exterior vessel, filled with the frigorific mixture; a wetted piece of bladder is then to be tied over the opening, or the cover is put on, to retain the mixture. The second frigorific material (for instance if quicksilver is to be frozen) consisting we will say, of muriate of lime and snow, are to be cooled by the mixture in the exterior vessel, by putting the muriate of lime into the conical glass vessel, together with the mercury contained in a thin glass tube; and surrounding the glass vessel, by filling the interior tin vessel with snow, or pulverized ice. When these materials have been cooled down to  $0^{\circ}$ ; the snow and ice may be mixed together by emptying the muriate of lime into the vessel containing the snow, and stirring the mixture with a glass rod to facilitate the solution of the salt, and to produce the requisite degree of cold. The number of apparatus I have sold to philosophical chemists, gives me reason to suppose, that they have proved useful.

FREDERICK ACCUM.

11, Old Compton Street,  
Soho.

Letter

## X.

*Letter from Mr. Accum, in answer to the Enquiries of a Correspondent respecting the Process for obtaining the Agustine Earth.*

To Mr. NICHOLSON,

DEAR SIR,

Mistake respecting the publication of the method of obtaining agustine earth.

YOUR correspondent, P. O. in the last number of your Journal, is correct, when he observes, that the process for obtaining agustine earth is not noticed in my system of practical chemistry, nor in any other work published in this country, and also that most of the books he quotes, were published a considerable time after this earth *was made known* by professor Tromsdorf; but he is mistaken in his opinion, that it was known in this country previous to the publication of the above works,

First account.

The method of separating this earth from the mineral which contains it, had not then been communicated to us through the usual channels of scientific information. The first account of the method of separating this earth I can find, is contained in a german work, entitled *Praktische Anleitung zur zerlegenden Chemie*, published by Professor Göelling, 1802. From which the following translation is made.

The process. Saxon beril pounded; boiled with potash; then fused;

Let a determinate quantity of the Saxon beril finely levigated, be boiled in a silver vessel, with three or four times its weight of potash, dissolved in a sufficient quantity of water; evaporate the whole to dryness, and fuse the mass.

softened by water; dissolved in muriatic acid; evaporated to dryness; diluted with water; and the filix separated; the fluid precipitated by carbonate of soda;

Soften the alkaline mass by the gradual addition of water, and when detached from the crucible, add to it muriatic acid till the whole is dissolved. Evaporate the solution to dryness, boil the mass in a sufficient quantity of water, and separate the insoluble residue [filix] by the filtre.

Decompose the fluid from which the siliceous earth has been separated, by gradually mingling it with a solution of carbonate of soda; collect the precipitate, and wash it repeatedly.

separate the alumine from the precipitate by potash which dissolves it and leaves the agustine earth.

When the precipitate obtained in the last process has acquired some consistence, transfer it into a flask containing a concentrated solution of potash. The alumine which was present

present in the mineral, will be dissolved\*, and the insoluble residue left, is the new earth called *Agustine*.

It is distinguished from all other earths by being absolutely insoluble in potash, soda, and ammonia, and all their carbonates. Nor can an union of either of the two first alkalies with *Agustine* be effected by fusion. It is soluble in acids, with which it forms salts, which have little or no taste. It is soluble in acids with equal facility after having been ignited, as when fresh prepared. It fuses with borax into a transparent colourless glass.

100 parts of the Saxon beril yielded Professor Tromsdorf 78,0 *Agustine*, 4,5 alumine, and 15,0 silica.

I am, Sir,

Your most obedient,

FREDERICK ACCUM.

11, Old Compton Street, Soho,  
15th October, 1803.

## XI.

*Letter from a Correspondent concerning the Method proposed by  
Mr. Carlisle for closing wide-mouthed Vessels.*

October 15, 1803.

SIR,

IN the last number of your Journal, published on the first of this month, page 68, I find a letter addressed to you by Mr. Carlisle, describing "a method of closing wide mouthed vessels intended to be kept from communicating with the air;" and in Plate V. Fig. 2, there is an engraving of the vessel re-

The method of  
closing vessels  
suggested in our  
last number

\* It is perhaps needless to state that the alumine may be separated from the alkaline solution, by saturating it with muriatic acid in excess, so as to neutralize not only all the potash, but also to dissolve the alumine, and then to decompose the obtained solution by carbonate of ammonia. Should glucine be expected, the carbonate of ammonia should be added in considerable excess; for the excess of ammonia retains in solution the glucine, and nothing but alumine will be thrown down. The glucine may be obtained by evaporating the solution to dryness and igniting the residue.

F. A.

com-

Was used by  
Buffon;

and by Le Cat;

and before all by  
Glauber.

commended by the above ingenious gentleman. But I must beg leave to observe that this, which in your Journal is denominated "a new method, by a jar, the cover of which fits into a groove with hog's lard," is in reality a very old method; for in the quarto edition of Buffon, published at Paris in 1749, Tome III. p. 192, you will find many such vessels represented, and *Fig. 4.* is precisely the same as the one given in your work. But Buffon was not the inventor of this method, for it is claimed by the celebrated Mr. Le Cat, who had glasses made upon this construction about the year 1739, and in 1748 sent to the Royal Society a description, which, with an engraving, may be seen in the 46th volume of the Philosophical Transactions, page 6.

But even the celebrated Mr. Le Cat was not the inventor, for old Glauber employed this method long before, and a plate and description may be seen by any one who will take the trouble to consult his "*Fornacum Philosophicarum, pars quinta,*" page 13, &c. &c. published at Amsterdam in 1661. The only difference is, that Glauber used quicksilver to fill the groove; Mr. Le Cat employed quicksilver or oil; and Mr. Carlisle recommends hog's lard. If you think these remarks worthy of a place in the next number of your Journal, they are much at your service; from an

OLD CORRESPONDENT.

## XII.

*Account of an Experiment for supplying Worm Tubs and other Refrigeratories by the assistant Pressure of the Atmosphere, which proved unsuccessful, on a large Scale; to which is added an Improvement for extending the useful Applications of the Syphon. By EDWARD HOWARD, Esq. F. R. S. In a Letter to the Editor.*

To Mr. NICHOLSON.

SIR,

Apparatus for  
more easily  
raising water in  
worm tubs, by  
atmospheric  
pressure.

THE method of supplying worm tubs and condensers, given by Sir Alexander Edelcrantz in your last number, induces me to trouble you with the result of an unsuccessful experiment, which I some time since made on much the same principle, and

and on a worm tub of a considerable size. Indeed it was my first intention to have made the worm tub part of the arm of a syphon; but, as warm water was constantly wanted in an elevated part of the premises, and as all the water used was raised by a lifting pump from an adjacent well, an opportunity appeared to present itself, both of economizing labour, and of making use of the warm water of the upper surface of the worm tub. To effect these objects, I made the pipe coming from the well immediately communicate with the tub, and annexed the pump, by means of another pipe, to the upper surface of the tub. By this construction, there was every reason to conclude that it would be practicable to pump off the warm water, and also, that whenever the pump was worked to supply the other demands of the laboratory, the water in the worm tub would be constantly changed without additional labour. It was further thought necessary to add a valve of safety to the upper part of the tub, and two stop cocks, one on the pipe leading from the well to the tub, and the other on the pipe leading from the pump to the tub, in order that by the regulation of these cocks, water might be had either immediately from the well, for other purposes of condensation, &c. or it might be drawn from the worm tub for processes requiring warm water, or for uses to which warm or cold water might be indifferently applied.

To give a better idea of the apparatus, I have subjoined the following outline, *Plat* XII. where A represents the worm tub; B a pipe leading from the well; C a pipe communicating immediately from the pump; D a valve of safety, E a pump; F a well; and G G stop cocks.

The apparatus when made upon a small scale, with a Woulfe's bottle and glass tubes, answered perfectly and promised to be a valuable acquisition. I wish I could relate the success of the same experiment made upon a large one: But, notwithstanding the best workmen in London were employed, they could not make the joints of the worm tub sufficiently tight to resist the pressure of the atmosphere for more than a few successive hours.

The apparatus succeeded perfectly in the small way; but failed on a large scale.

I should not, Sir, have offered to you the result of an unsuccessful experiment were it not from an apprehension that the worm tub recommended by Sir Alexander Edelschantz might be no less difficult to construct than the one I have described.

Allow



Improvement in  
the syphon.

Allow me, Sir, to take advantage of this opportunity to communicate what I believe to be an improvement of the syphon. It was made at the time I had an idea of applying the principles of a syphon to the worm tub: Although it may not be applicable to this purpose, it will be found exceedingly useful in cases where it would be disagreeable, dangerous, or impossible, to exhaust the syphon by the common mode of sucking out the air, if I may be allowed such an expression. I think it even probable that syphons of considerable dimensions may be introduced for emptying ponds, or for lowering the water in mill-dams or canals; for it is without doubt desirable to avoid cutting through an embankment, and a very large instrument of the new construction would be thus put in action with great ease. The improvement, Sir, I have thus ventured to speak of consists merely in enlarging the exhausting pipe to the same calibre as the rest of the syphon; in elevating it a little, and in opening its mouth like a funnel. See *Fig. 3. Plate XI.*

It is scarcely necessary to point out that, to use such a syphon, the short arm is, as usual, to be immersed in the liquor intended to be operated upon, and the aperture of the long arm to be closed whilst the whole instrument is to be filled through the funnel with some of the same liquor.

I am,

SIR,

Your obedient humble servant,

EDWARD HOWARD.

### XIII.

*A Method of equalizing the Motion of a Steam Engine without the Assistance of a Fly Wheel. By Mr. ARTHUR WOOLF, Engineer. Communicated by the Inventor.*

Equalizing mechanism which allows the engine to set off or stop in any part of the stroke.

THE mechanism here presented as a substitute for the fly, possesses the advantage of equalizing the motion, with the power of being stopped and set to work at any part of the stroke, the utility of which, in mines, collieries, and other works, will be immediately seen by those conversant in such undertakings.

*Plate*

*Plate XI. Fig. 1.* A represents part of the engine beam; B the connecting rod; C the crank arm; D a cog-wheel, working into another cog-wheel F, of half the size; F a crank arm on the shaft of the small wheel; G a cylinder closed at bottom, in which a solid or unperforated piston moves, leaving a vacuum beneath. This acts simply instead as a weight on the crank F, by the constant pressure of the atmosphere; and the diameter of the piston must be such as nearly to equal one third of the power of the engine.

In *Fig. 2.* the outer circle is the line described by the crank; the circumference of the inner circle is equal to twice the diameter of the outer, and the square has the same circumference; this last exhibits the inequality still remaining, which by this method is reduced to about one fifth; but by the assistance of a small fly on the second motion, the effect will become nearly the same as that of a rotative engine, with the advantages here mentioned.

The same motion may be applied to a pump, but in this case the two cranks must be horizontal at the same time.



## XIV.

*Improvement by which the additional Arc in Mr. Ezekiel Walker's reflecting Quadrant is rendered unnecessary. In a Letter from the Inventor.*

To Mr. NICHOLSON,

SIR,

ANY one who has a just idea of the reflecting quadrant, <sup>Improvements in Mr. E. Walker's reflecting quadrant.</sup> described on page 218 of the fourth volume of your Journal, will perceive that one half of the arc of that instrument is appropriated solely to the rectifying of the second horizon glass. This method of adjusting is as good as any other that I have to propose, but it is attended with the inconvenience of adding to the size of the instrument, and consequently to its weight.

The following method of adjusting the second horizon glass will reduce the instrument to an octant, which will still possess the same property of measuring any angle less than 120° by the fore observation.

First

Improvement  
in Mr. E. Wal-  
ker's reflecting  
quadrant.

First, let a small mirror be fixed upon the top of the index glass. This rectifier must be placed parallel to the first horizon glass, when the index stands at  $90^\circ$  on the arc, consequently the index glass and rectifier will form an angle of 45 degrees. Secondly, let the two horizon glasses stand as high above the plane of the octant as the rectifier, and the instrument is ready for use.

After the first horizon glass has been adjusted, bring the index to  $90^\circ$ , and the rectifier will be parallel to the horizon glass, if the index glass and rectifier form an exact angle of  $90^\circ$  on the arc; but if they do not form that angle, the index will show the error. Then to determine the error of the second horizon glass, let the index be brought to 0, and the rectifier will in that situation perform the same office as the index glass in the quadrant, when the index stands at  $90^\circ$  at N; but as this has been fully explained in the description of that instrument, it need not be further insisted on here.

An octant of five inches radius, constructed on these principles, would be exceedingly portable, and so strong as not to be easily deranged by carriage; and these are properties which may recommend it to the attention of the traveller by land, particularly if he visit those latitudes, where the altitude of the sun sometimes exceeds 60 degrees.

These two glasses may also be added to the sextant, without depriving it of any of the valuable properties which it now possesses: and this additional apparatus need only be used in taking such angular distances as are beyond the power of that instrument,

I am,

SIR,

Your's respectfully,

E. WALKER.

*Lynn Regis, October 15th. 1803.*

*Plate XI. Fig. 3.* A B represents the index glass; C D the rectifier; m the horizon glass; n the second horizon glass.

## SCIENTIFIC NEWS.

*Abstract of Cit. SEGUIN's Inquiries concerning Fermentation\*.*

IN his first paper Cit. Seguin explains the plan of inquiry he has undertaken concerning fermentation in general; and more particularly concerning the making of beer, wine, cider, malt and melasses spirits, &c. In his second his object is to prove, that fermentation is not produced by a substance *sui generis*, but by a combination of circumstances.

Fermentation not produced by a peculiar substance, but by a combination of circumstances.

He shows, that, in the case of clear liquors fermenting, the true solvent of the fermentescible cause, whatever it be, is water, and not the saccharine matter; that the continuance of contact, and the presence of sugar, are by no means necessary for the solution of any fermentescible principle in the yeast; that this solution is made by water in a very small quantity, it is true, but almost suddenly, and even in the ordinary temperature of the air; finally, supposing sugar also to possess the property of dissolving any given fermentescible principle, it would be impossible to demonstrate this, since, to render it perceptible, the sugar must previously be dissolved in water.

Water the true solvent of the fermentative cause, and this speedily, and without the presence of sugar being necessary.

*Additional Experiments of Mr. RITTER, of Jena, on Galvanic Phenomena†.*

NATURAL philosophers will learn with pleasure, that Mr. Ritter continues his elegant experiments on a subject, to the progress of which he has so greatly contributed. As they all relate to a known theory, we shall content ourselves with giving the results as communicated to us by Mr. Orsted, leaving to the experimentalist the task of proving them with all the requisite minuteness.

The object of Mr. Ritter being to compare the electricity of Volta's pile with that of electrical machines, he considers successively the intensity of electricity, chemical action, spark and shock, in the pile.

Mr. R.'s object to compare Volta's pile with electrical machines.

\* Bulletin des Sciences, No. 75.

† Bulletin des Sciences, No. 77.

Electricity positive at one pole of the pile, negative at the other, diminishes between them, null in the centre. The time necessary for charging a battery, an inaccurate measure of intensity.

+ Electricity of the pile disposes metals to combine with oxygen, — with hydrogen.

+ pole armed with gold leaf, — with charcoal, on forming a communication the gold leaf is burned; if the charcoal be on the + side, it burns, and the gold is melted.

— pole, brought into contact with quicksilver leaves a mark on its surface different from that of the +.

The effects of the pile on the animal body all reducible to expansions and contractions.

The + pole increasing the bulk of parts, the — diminishing it. Effects on the tongue,

and on the pulse. The expansion occasions a sense of heat, and vice versa.

To the eye the

As to the intensity, we know that the electricity is positive at one of the poles of the pile, and negative at the other; it has been shown likewise, that it diminishes between these two extremes, so as to be null in the middle of the pile. Mr. Ritter sought to compare the degrees of intensity at these two poles, and those of different piles. This he attempted by determining the time necessary for charging a given battery; but this method is inaccurate, and no certainty can be attained in this respect, but by means of the electrical balance.

According to Mr. Ritter, the action of the positive pole of the pile disposes metals to combine with oxygen, and that of the negative pole disposes them to combine with hydrogen. If the positive pole be armed with a gold leaf, and the negative with a bit of charcoal, on forming a communication between these two substances the leaf of gold burns with a brilliant light, and the charcoal remains untouched: but if the charcoal be placed on the positive side, and the gold on the negative, the charcoal burns, and the gold is melted. If the negative pole be brought into contact with the shining surface of quicksilver, it leaves a trace different from that produced by the positive pole.

Mr. Ritter asserts, that all the effects of the pile on the animal body are reducible to expansions and contractions. All the parts of the human body assume an increased bulk on the contact of the positive pole, and contract on the contact of the negative: for instance, the action of the positive pole on the tongue produces there, at the expiration of a few minutes, a slight elevation, whereas the negative pole occasions a little depression. If the same person touch the two poles with the two hands wetted, the intensity of the pulse is increased in the hand in contact with the positive pole, while its strength is diminished in the other, but the number of pulsations continues the same in each. The expansion thus produced in the organs, is attended with a sensation of heat, the contraction with a sense of cold.

If the eye be made to communicate with the positive pole, it sees objects red, larger, and more distinct; in contact with the negative pole it sees them blue, smaller, and more confused. The tongue receives from the positive pole an acid taste, from the negative an alkaline. The ear being in con-

tact

and with the former, all sounds seem more grave: with the latter, more acute.

In general the two poles of the pile produce opposite effects.

Such are the results of Mr. Ritter's experiments. We have no opportunity of verifying their accuracy; but their singularity, their number, and particularly the ingenuity of their author, lead us to presume, that this account of them will be read with pleasure.

SOCIETE PHILOMATH.

+ pole makes objects red, large & distinct; the —, blue, small, and confused.  
+ gives an acid taste,— alkaline.  
+ deepens sounds.  
— lightens them.  
The two poles generally produce opposite effects.

*Abstract of some Remarks on the Acetite of Lead, by*  
Cit. THENARD \*.

A MANUFACTURER of acetite of lead was obliged to stop his works, being no longer able to make the salt crystallize in needles, but always obtaining it in laminæ, which induced purchasers to refuse it; Cit. Thenard inquired into the cause of this phenomenon, and soon perceived, that it was owing to the proportions of the constituent principles of the salt. He succeeded in forming a salt perfectly similar to it, by boiling in water a hundred parts of the acetite of lead of the shops, with a hundred and fifty parts of litharge well dried, and deprived of carbonic acid by means of fire. Analysis corroborated the existence of two species of acetite of lead; one, long known, consists of oxide of lead 0.58, acetous acid 0.26, and water 0.16, the other, which has hitherto escaped the notice of chemists, contains oxide of lead 0.78, acetous acid, 0.17, water 0.05.

The former of these salts has an excess of acid, and a strongly saccharine taste; crystallizes in needle prisms, which appear to be hexagonal, and terminated by hexaedral pyramids; undergoes no alteration in the air; is very soluble in water, and forms with it a solution feebly precipitable by carbonic acid. The latter, on the contrary, is neutral; has a less perceptible saccharine taste; affects a lamellated figure; is soluble in vinegar, and then exhibits on evaporation the needle form;

A manufacturer obliged to stop his work, because his acetite of lead was always in the form of laminæ.

Two species of acetite of lead. The common one that with excess of acid.

The new or neutral acetite.

\* Bulletin des Sciences, No. 77.

Fine white lead  
may be prepared  
from it.

effloresces slightly in the air; is much less soluble in water, but forms with it a solution abundantly precipitable by carbonic acid. This precipitate is very white, forms a paste with oil, and by extracting the carbonic acid from chalk by means of fire, it would perhaps be possible to prepare a fine white-lead by these means.

Advantages of  
the discovery.

The value of the discovery of this salt, will be readily perceptible. It not only brings us acquainted with a new substance interesting in a scientific view, as it affords us a fresh proof, that the proportions of the constituent principles of salts may vary greatly; but it is likewise of importance in the art of physic, in which salts of lead are daily employed, and which may require one containing a large proportion of oxide; and it is of consequence to the arts in general, as it affords a new method of obtaining a fine white-lead, and particularly to that of manufacturing acetite of lead, on the processes and products of which it throws great light.

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*The Arachis Hypogæa, or Ground Nut of the West Indies, cultivated in France for its Oil.*

Arachis hypo-  
gæa cultivated  
in France.

IN the departments of Landes and PHERault in France, lat.  $43\frac{1}{2}^{\circ}$  to  $44\frac{1}{2}^{\circ}$ , an oily plant, called arachis, of the family of lentils, (*arachis hypogæa*) begins to be cultivated. It was brought by the Spaniards from Mexico, and was introduced by the French from Spain. An ounce of the oil of this plant, with a wick a line and half in diameter, burned nine hours and twenty-six minutes; an ounce of olive oil under similar circumstances, lasted only eight hours. Thus the oil of the arachis has the advantage of more than one eighth over olive oil; and it has more or less over every other kind of oil. It is an excellent substitute for olive oil for every domestic purpose, and is preferable to all other oils for the manufacture of soap. The seed yields nearly half its weight of oil.

Affords an oil  
excellent for  
lamps,

and other pur-  
poses.

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\* \* The enquiries of a correspondent, respecting the method of experiment adopted by Dr. Irvine, will be answered in our next.

*Perspective View of Mr. Hoar's method of supporting Timbers.*

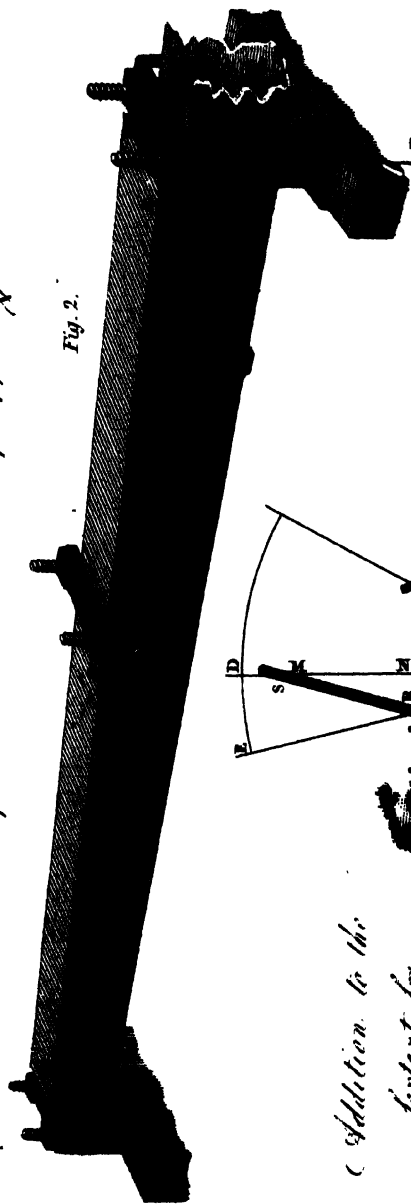


Fig. 2.

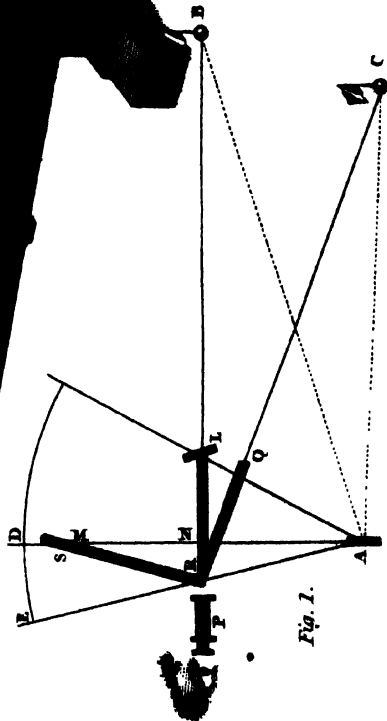
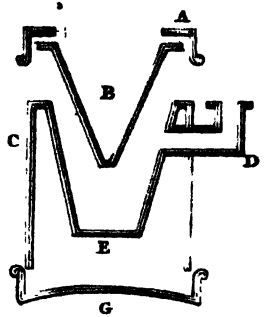


Fig. 1.

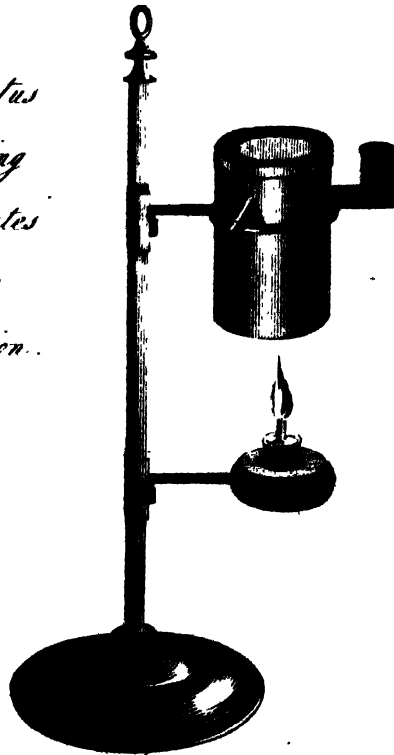
*(Addition to the  
tent for  
aerostatic observations.)*







*Apparatus  
for drying  
Precipitates  
and for  
Congelation.*





*Mr. Woolf's  
Rotatory Apparatus.*

Fig. 1.

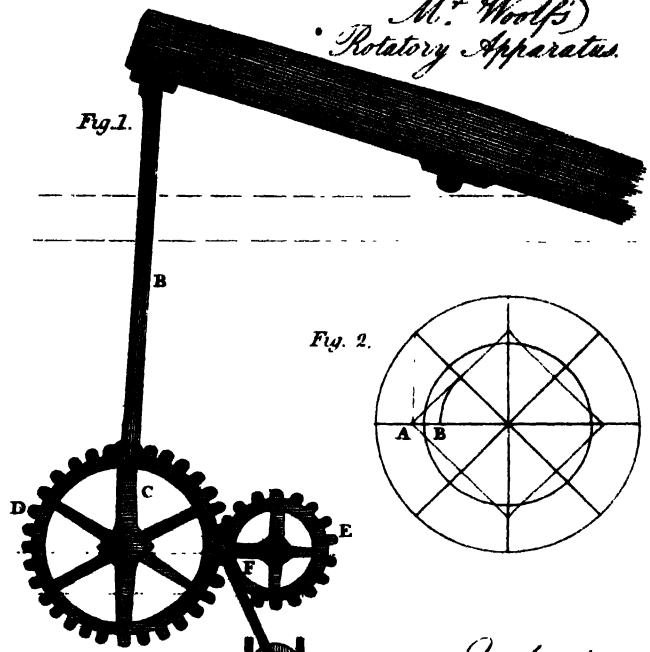
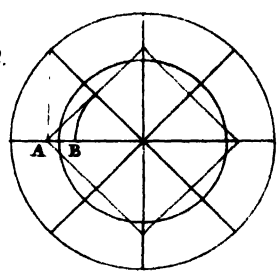


Fig. 2.



*Quadrant  
of Mr. E. Walker*

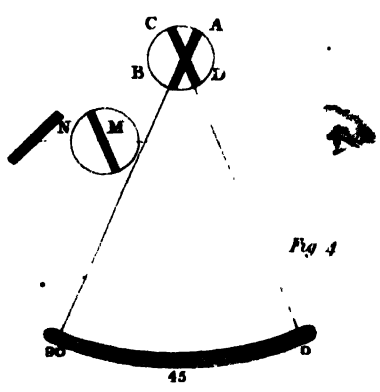


Fig. 4

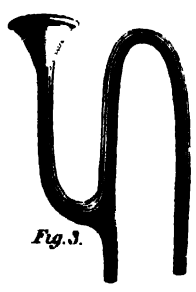
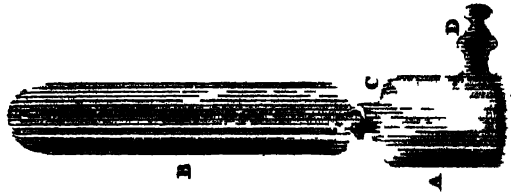


Fig. 3.

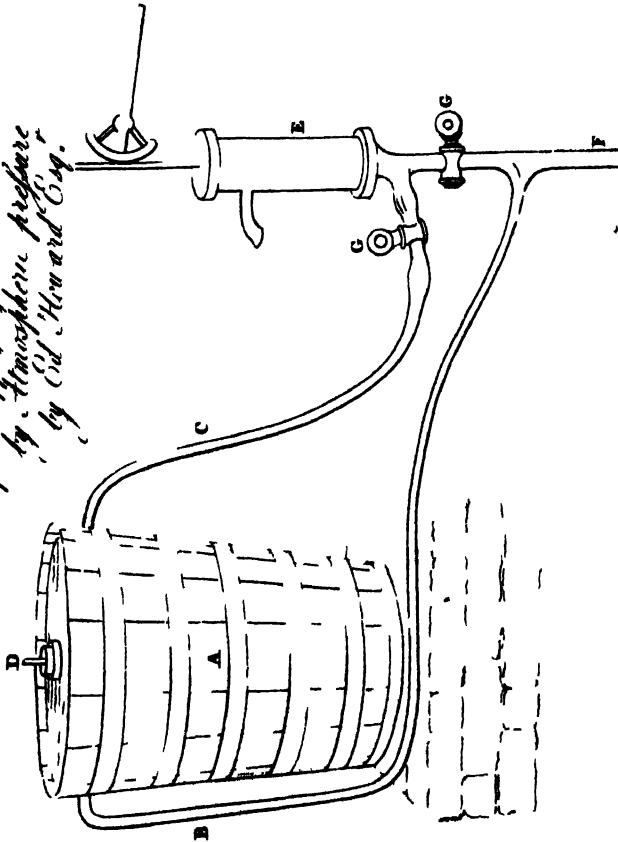
*Improved Siphon.*



*Dr. Hopes  
Endometrie  
Apparatus*



*Experiment  
for supplying them with  
atmosphere prepared  
by Col. Howard Eng.*





A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

DECEMBER, 1803.

ARTICLE I.

*Letter from ANDREW DUNCAN, M. D. F. R. S. E. containing Experiments and Observations on Cinchona, tending particularly to shew that it does not contain Gelatine.*

To Mr. NICHOLSON.

S. I R,

HAVING been long engaged in a series of experiments on the astringent substances employed in medicine, I was particularly interested with the "Abstract of a Memoir on the Febrifuge principle of Cinchona," contained in the last number of your excellent journal. The presence of gelatine in cinchona, was so incompatible with experiments I had formerly made, that I was strongly inclined to believe, that Seguin (than whom no one should be better acquainted with the combinations of tannin and gelatine) had been misled, either from having examined cinchona which had been adulterated, or from some other accidental cause. To satisfy myself I immediately proceeded to the unerring test of experiment, which has convinced me that cinchona does not contain gelatine, but some other principle not yet sufficiently examined, which agrees with gelatine, in forming with tannin, a precipitate comparatively insoluble in water. At the same time it is fair to remark, that my experiments were made with the in-

*Presence of gelatine in cinchona, a mistake.*

*But it contains some other principle, forming an insoluble precipitate with tannin.*



Infusion and tincture of cinchona, precipitate solutions of tan, gelatine, and sulphate of iron.

fusion and tincture of cinchona, containing all the soluble principles of that substance, whereas Seguin's observations are said to be derived from the examination of the isolated febrifuge principle, of which he gives the following characters: "It precipitates the solution of tan, but not the solutions of gelatine and sulphate of iron." On the contrary my experiments teach me, that the entire infusion and tincture of cinchona, precipitate the solution of tan, and also the solution of gelatine slightly, and the solution of sulphate of iron copiously. But as the two last precipitates may be reasonably ascribed to the action of other principles contained in my infusion and tincture of cinchona, I shall not insist upon them, but proceed to show that, although cinchona actually does precipitate the solution of tan, yet it does not contain gelatine.

#### Experiment I.

Experiments in proof of this. Infusion of galls precipitated by infusion of cinchona.

(a) An ounce of infusion of galls was saturated, by adding to it in different portions, an ounce and a half of infusion of cinchona. The mixture was white and turbid, with a loose light precipitate.

(b) On filtration the fluid passed almost colourless, and perfectly transparent.

(c) The precipitate when dried, weighed five grains. It had a yellow colour, and an opaque earthy appearance, was extremely friable, and did not adhere to the filtering paper.

Gave a further precipitate with gelatine.

(d) The filtered fluid gave no further precipitate with solution of cinchona, but with half an ounce of solution of gelatine, containing six grains of gelatine in each ounce, it produced a copious precipitate, and was saturated.

(e) The precipitate, when separated by filtration, and dried also, weighed five grains, but was hard and brittle, adhered strongly to the paper, had a yellow colour, and exactly resembled a resin in appearance.

#### Experiment II.

Infusion of galls precipitated by gelatine.

(a) An ounce of the same infusion of galls was saturated by an ounce and a half of the same solution of gelatine. Immediately a very copious, whitish, tenacious, and adhesive precipitate was formed.

(b) On filtration the fluid passed very slowly, and even after repeated filtration, still retained a slight degree of opaline bluishness.

(c) The

(c) The precipitate when dried, weighed fourteen grains and a half. It had a brownish yellow colour, was transparent, and had a resinous appearance and fracture. It was also hard and brittle, and adhered strongly to the filter. In every particular it resembled the precipitate produced in the former experiment (*Exp. I. c.*) by gelatine, after the infusion of galls was completely saturated by cinchona.

(d) In the filtered liquor (*Exp. II. b*) infusion of cinchona produced no change.

Infusion of cinchona gave no farther precipitate.

### Experiment III.

To an ounce and a half of the same infusion of cinchona, half an ounce of the solution of gelatine was added. It produced only a slight degree of turbidness, and changed the colour of the infusion from a pale greenish to a reddish yellow colour. When filtered, it passed perfectly transparent, and the bottom of the filter was covered with a red varnish; but it had gained only one grain in weight. In other experiments with larger quantities and stronger infusion of cinchona, the presence of tannin was more strongly indicated.

Infusion of cinchona with gelatine gave slight signs of tannin.

### Experiment IV.

Infusion of galls was not affected by rectified spirits of wine, in which isinglass had been long infused.

Infusion of galls not affected by isinglass dissolved in alcohol.

### Experiment V.

(a) A tincture of cinchona was prepared by infusing it in the same rectified spirits. After it was filtered some resin was separated by precipitation with water and filtration.

Tincture of cinchona precipitated by water,

(b) With infusion of galls this tincture gave a copious precipitate, exactly resembling that produced by the same reagent and infusion of cinchona. (*Exp. I. c.*)

and by infusion of galls,

### Experiment VI.

With tincture of galls the same tincture of cinchona gave no precipitate.

Tincture of galls and of cinchona gave no precipitate,

### Experiment VII.

In the mixed tincture (*Exp. VI.*) a copious precipitate was still diluted by produced by diluting it with water.

water.

*Experiment VIII.*

Carbonate of  
potash precipi-  
tates solution  
of gelatine,

A solution of carbonate of potash produced a copious white flaky precipitate in the solution of gelatine, which was soluble in boiling water, but was not precipitated from the solution by infusion of galls, until some acid was added.

*Experiment IX.*

but not infu-  
sion of cin-  
chona.

The solution of carbonate of potash changed the colour of the infusion of cinchona to a fine red, without disturbing its transparency.

Difference be-  
tween gelatin  
and cinchonin.

These facts seem to me sufficient to prove the difference between gelatine, and the new principle in cinchona, which for the sake of convenience, I shall venture for the present to denominate cinchonin.

Gelatin forms a  
jelly with water,

Gelatine is soluble in water, and the solution is disposed to gelatinize. Six grains of isinglass, dissolved in one ounce of water, form with it at temperatures below 60° Fahrenheit, a jelly of considerable firmness. From its solution in water, gelatine is precipitated by alcohol, and a solution of carbonate of potash. It is precipitated also by tannin, and the precipitates form a hard brown transparent mass.

and is precipi-  
tated from it  
by alcohol, car-  
bonate of pot-  
ash, and tannin.

Cinchonin does  
not form jelly  
with water, is  
not precipi-  
tated by carbo-  
nate of potash,  
is soluble in al-  
cohol, and com-  
bines with  
tannin.

Cinchonin is soluble in water, but gives it no tendency to gelatinize. From its solution in water, it is not precipitated by a solution of carbonate of potash. It is soluble in alcohol; it combines with tannin. The compound is soluble in alcohol, but forms, when water is added, or used as a menstruum, a friable opaque yellowish precipitate; but cinchonin does not separate even from a watery solution of tannin, all that is precipitable by a solution of gelatine.

ANDREW DUNCAN, Jun.

Edinburgh, 30th Oct. 1803.

## II.

*Letter from a Correspondent, containing Disquisitions on the Phantasms of NICOLAI, and other Derangements of the Animal System.*

To Mr. NICHOLSON,

SIR,

THE account published in your last number by Mr. Nicolai, of his seeing spectres, appears not only to admit of an explanation on some of the laws of vision with which we are already acquainted, but may also lead to some rational account of the grounds, on which the belief in apparitions, which has so generally prevailed in all ages of the world, may be founded.

*The phantasms of Nicolai may be explained,*

If we look for a moment at the sun, and afterwards turn our eyes on the blue sky, or throw them upon the ground, we perceive a black spot of the apparent size of that luminary.

*—from the impressions of visible objects on the eye.*

This spot will by degrees assume a faint green colour, then becomes red; and if we pay attention to these phenomena, we shall find the green and red colours alternate, becoming gradually more faint, till they wholly disappear.

If the eye be directed for some time towards a window, and then covered by the hand, the bars will first appear luminous, and the squares dark, and then the contrary, and this will alternate till the whole image gradually fades away.

*Instance of a window.*

These are termed ocular spectra, and have been supposed to depend on the alternate tension and relaxation of the fibres of which the retina are composed. They resemble the trembling of an over-fatigued muscle, in which one set of fibres attempts to relieve the exertions of another. These phenomena are most obvious after the eye has been fatigued by long continued exertion. They are more readily observed in the evening than the morning, and the spectra of the setting are more easily caught than of the meridian sun.

*These ocular spectra are most obvious in the state of debility or fatigue.*

In certain states of bodily debility, whether produced by the absence of volition, or by disease, they are known spontaneously to occur. In the former state, which may be termed reverie, when a person attends slightly to the impressions of his senses, without attempting to regulate them by his will, as when a person looks carelessly in the fire during twilight, the

*Spontaneous occurrence of spectra in the state of reverie.*

infinite

Swedenbourg  
perpetually in  
this state.

infinite variety of fantastic forms that pass before the eyes is familiar to every one. These fancies are however, in some measure, influenced by the train of thinking a person is accustomed to pursue. Swedenbourg appears to have been perpetually under the influence of this kind of reverie, except when employed in writing an account of what had previously occurred to his mind. Had he been constantly occupied in active bodily exertion, perhaps it would have dissipated his phantasies.

Why dying  
persons pick the  
bed-clothes, and  
imagine they see  
demons or dark  
figures.

Picking the bed cloaths, which appears to occupy the attention of those who are labouring under the debility preceding death, probably arises from parts of the retina becoming insensible of the impression of light, which produces the sensation of something dark lying on the bed-cloaths, which they are desirous to remove. The enfeebled mind may not unfrequently transform these into the figures of demons. The pursuit of the dark spot formed by the insertion of the optic nerve, which in that state is mistaken for a reality, may also give occasion to this action so frequently observed among the dying.

Nicolai evidently  
indulged the  
state of reverie,

Mr. Nicolai informs us, that he was in the habit of forming vivid representations to himself not only of whole scenes of comedies, but even of the peculiar dresses, forms, and complexions of those who performed them; that is, he accustomed himself earnestly to attend to these ideal exertions of his own imagination. In the next place he tells us, that he first began to see spectres after having laboured under a nervous fever, and great trouble of mind. For the melancholy cast of his earlier visions, or the appearance of dead bodies, his dejected state of mind accounts. That appears however to have soon subsided. The diseased state of the retina, consequent to his fever, seems to have continued longer.

The phantasms  
were of things  
before seen.

This state appears to have been such as to render him sensible of the spectra of the things he was in the daily habit of seeing, such as men, horses, dogs, &c. for he does not say he ever saw any thing uncommon. What this particular condition of the retina might consist in, it is impossible to determine. It may have been a combination of weakness with excess of sensibility. The spectra of the bodies he had seen involuntarily recurred to his attention, but not with such strength as to prevent the more forcible impression of what was in reality passing before

before him. The notion of hearing the phantoms speak I should refer to an affection of the organ of hearing, similar to what took place in the eye. It is evident they were both removed by a slight diminution of the quantity of circulating blood. Audible delusions.

Many stories of apparitions may, in my opinion, be accounted for on similar principles. A person fixes his eyes intently on the face of an expiring friend illuminated by the light of a candle, perhaps with the intention of taking a last farewell; soon after, going into the dark, the spectrum of this luminous appearance occurs to the fatigued eye, and he thinks he perceives the dying man he had just left, standing before him. Phantasms of departed friends.

On these principles we may venture to correct an error in the general representation of our justly popular play of Hamlet. The ghost should only appear once. This single appearance makes so strong an impression on the mind of Hamlet, which, together with an habitual melancholy, was debilitated by care and vexation, that whenever afterwards he thinks seriously on his father, the spectrum of the ghost recurred to his eye, as he himself informs us, when he tells Horatio he sees his father, and is asked where, he says, "*in my mind's eye.*" Correction of the representation of Hamlet.

These cursory remarks, Sir, are in some measure written to evince how much better it is to attempt at least, to account for phenomena on principles already known, than to hunt for a new cause for every uncommon appearance. But what kind of philosophising can we expect from a man, who quotes such canons as the following, for rules of philosophy? "That knowledge derived from experience is merely empirick, and therefore not to be depended on." "That observation should not be admitted in theoretical philosophy." What is theoretical philosophy? After the existence of Bacon, of Newton, and of Locke, who could have expected to live to see the whole thinking part of a nation puzzling themselves about opinions, which, if they admit of any description, may be characterized as a jumble of the abstractions of Aristotle, with the ideal system of Berkley? Concluding remark.

A STUDENT.

## III.

*Experiments on the Substance vulgarly called Gum Kino. By C. T. VAUQUELIN \*.*

- Kino not a gum.** THE name given to this substance is by no means suited to it; and were it not a common practice, to give names to things before we are acquainted with their nature, it would be inconceivable how it should have been called a gum, having neither the physical nor chemical properties of one.
- Whence it is obtained not known.** Neither have we any accurate knowledge of the tree or of the country that produces it; but it appears to have been first brought to Europe by the English, who made known its medicinal properties, and introduced it into our shops.
- Said to be from the pau de sangue of Africa.** It is called in trade *kino* or the *gum-resin* of Gambia. Dr. Oldfield, who made it known to Fothergill, termed it the true gum of Senegal. In the Medical Observations and Inquiries, it is said to be brought from Africa, and the tree that furnishes it to be called by the natives *pau de sangue*.
- Used in medicine as a tonic.** As a medicine it is used in the form of bolusses, lozenges, aqueous infusion, and spirituous tincture, as a tonic and astringent.
- Yields an aqueous fluid, an oil, carbonic acid, and carbonated hydrogen.** Subjected to the action of fire, it melts and swells up considerably: yields at first a clear liquor, which in a few instants becomes coloured; a light and nearly white oil then passes over, which in the course of the process becomes coloured and heavier than the aqueous product. A small quantity of carbonic acid is likewise formed, with a large quantity of carbonated hydrogen gas.
- Its oil.** The oil produced in this operation unites with caustic fixed alkalis, and forms a deep red liquor, that becomes of a dull green on exposure to the air.
- Its aqueous product.** The aqueous product is not acid, but has an acrid burning taste, owing to a portion of the oil retained in solution; and potash separates from it a large quantity of ammoniac.
- Its residuum.** Twenty grammes distilled with a strong heat left eight and half of a very bulky coal, marked with the colours of the rainbow; and this coal afforded seventy-two centigrammes of ashes, consisting chiefly of lime, silica, alumina, and oxide of iron.

Abridged from the *Annales de Chimie*.

\* Kino

Kino is little soluble in cold water, but much more in hot, though a portion of it is insoluble. The solution is slightly acid: alcohol does not precipitate it, but separates some reddish flocks; when made with boiling water it grows turbid on cooling, and deposits a brown red precipitate.

Its solution in water acidulous.

A saturated solution is precipitated by mild alkalis, but water in sufficient quantity re-dissolves the precipitate.

Precipitated by mild and caustic alkalis,

Caustic alkalis likewise precipitate it, but if added in excess re-dissolve the precipitate.

Glue dissolved in water forms a very considerable rose-coloured coagulum with the solution of kino; and if the quantities be such, that both substances are saturated, the supernatant fluid will be nearly colourless.

and coagulated by glue.

Though these appearances indicate the presence of tannin in kino, it does not precipitate ferruginous salts black, but of a beautiful deep green, scarcely alterable by exposure to the air. This property it has in common with the infusions of cinchona and rhubarb; whence it is probable, that these three substances contain a principle of similar nature.

Its tannin precipitates iron green.

Contains a principle common to i, bark, and rhubarb.

This principle, whatever it be, is very destructible: for, if a little oxygenated muriatic acid be poured on the precipitate it forms with iron, this loses its colour, and does not re-appear on the addition of an alkaline carbonate, which produces only a red oxide of iron.

This principle very destructible.

The solution of kino copiously precipitates acetite of lead of a yellowish grey; nitrate of silver of a reddish yellow, and tartre of antimony of a yellowish white, but much more copiously than the infusion of tan or of cinchona; which seems to indicate, that it would be a better antidote in cases of persons poisoned by this metallic salt.

Precipitates some metallic solutions.

Useful as an antidote.

Wool and cotton being boiled in a solution of kino, and then dipped in a bath of sulphate of iron, appeared on immersion of a bottle-green; but being washed and dried, the colour became a blackish brown. It was very durable.

Dyes wool and cotton a blackish brown.

Hot alcohol dissolves kino very well, all but a small portion. Water renders the solution a little turbid, but precipitates nothing.

Dissolves in hot alcohol.

The portion insoluble in alcohol, nearly a fourth of the whole, has neither the bitterness nor astringent taste of kino; but, on the contrary, is rather mucous and sweet. It easily dissolves

Contains a portion of gum.



in hot water, and gives it a fine red colour. It is precipitable by alcohol; but neither by glue, nor by any metallic solution. On burning, it diffuses a smell resembling that of gum.

This favours the solution of the other principle in water.

I suspect the presence of this substance favours the solution in water of the principle soluble in alcohol; for the latter is less soluble in water when separated from the former; and, if the quantity of water necessary for dissolving the astringent part be not employed in the first instance, what is left requires a greater proportion of water.

Water dissolves  $\frac{2}{3}$ , alcohol most of the remainder.

Four litres of water, used at different times, left near twenty grammes out of a hundred of kino undissolved. The residuum grew soft like a resin in boiling water, and all of it, except seven decigrammes, was soluble by alcohol, to which it imparted all the properties before observed in the astringent matter.

Sulphuric acid renders it less soluble.

Sulphuric acid diminishes the action of water on kino, instead of increasing it, as it does with respect to the resinous part of cinchona.

It tans leather.

It is capable of being used for tanning leather.

Neither a gum, nor a gum-resin, but chiefly a species of tannin.

From what has been said it appears, that the greater part of kino consists of tannin, and is neither a gum, nor a gum-resin. But there is a slight difference between it and the tannin of galls and oak bark, which precipitate iron of a blue black, while kino precipitates it green, in which it resembles cinchona and rhubarb. If therefore it were to become plentiful and cheap, it might be employed for all the purposes for which astringent vegetables are commonly used.

#### Addition.

Dr. Duncan first asserted it to be an extract.

Mr. Vauquelin is not the first who discovered the common error respecting kino. In the new Edinburgh Dispensatory, Dr. Duncan has entered pretty fully into the subject, and asserts it to be in reality an extract. He adds, that what we have now in the shops is not brought from Africa, but chiefly from Jamaica. In a private letter he informs me, that this is an extract of the *coccoloba urifera*, or sea-side grape; while the finest kino of the shops, and what from some circumstances he supposes was the first analyzed by Vauquelin, is the product of different species of *eucalyptus*, particularly the *resinifera*, or brown gum tree of Botany Bay, from which country a parcel was imported some years ago.

Obtained from the sea-side grape,

and gum-tree of Botany Bay.

## IV.

*Extract of a Letter from DR. PRINCE, respecting his  
Air-Pump.*

IT is unnecessary here to enter into the merits of Dr. Prince's air-pump, as they were noticed in the 1st. Vol. of the Philosophical Journal, 4to. edit. p. 130. The purpose of his letter is to defend himself against the misrepresentations of the writer of the article pneumatics, in the Encyclopedia Britannica; which he has done in the supplement to the edition of that work reprinting in America, where it may be presumed it will meet the eye of but few English readers.

The Encyclopedia says, "great inconveniences were experienced from the oscillations of the mercury in the gauge. As soon as the piston comes into the cistern, the air from the receiver immediately rushes into the barrel, and the mercury shoots up in the gauge, and gets into a state of oscillation. The subsequent rise of the piston will frequently keep time with the second oscillation, and increase it. The descent of the piston produces a downward oscillation, by allowing the air below it to collapse; and by improperly timing the strokes, this oscillation becomes so great, as to make the mercury enter the pump."

This, Dr. Prince observes, is a very singular account of the working of the American air-pump. It seems to be founded on experiment, yet it is contradicted by numerous experiments performed with the original pump, and with one on the same construction, made by the late Mr. G. Adams in London. Many scientific and respectable persons were witnesses, that no such extraoscillations were produced by it; but that the mercury rose in the gauge in the same manner as it did in a double-barrelled pump of the common construction made by Nairne. Add to this, Mr. Adams, who made the first pump in England on this plan, mentions no such effect, either in his letter to the inventor, or in his public account of it; nor does Mr. Jones, who has since made pumps on this plan, and given an account of their exhausting power, which he says, in a letter to Dr. Prince, is fully equal to that of Cuthbertson's.

To

Valves said to be put in the piston to prevent downward oscillation ;

To prevent the downward oscillation, which, Dr. Prince observes, could only occur in a single-barrelled pump, and which he obviated by using two barrels, the Encyclopedia says, " valves were put into the piston ; but as these require force to open them, the addition seemed rather to increase the evil, by rendering the oscillation more simultaneous with the ordinary rate of working." If, replies the Doctor, such an evil were produced by the descent of the piston, it is difficult to conceive, how putting valves into the piston could have increased it. They could not increase the evil, unless they increased the resistance to the air under the piston. But it must be a strange assertion, that a piston with a *valve* in it will give more resistance to the air than a *solid* piston.

but valves could not increase its resistance.

Said to be difficult of execution ;

Another objection is : " it appears of very difficult execution. It has many long, slender, and crooked passages, which must be drilled through broad plates of brass, some of them appearing scarcely practicable. It is rare to find plates and other pieces of brass without air-holes, which it would be difficult to find out and close, &c. Now the fact is, there is not so much pipe and duct-work in the American air-pump, by more than one half, as in Mr. Nairne's improved pump of Smeaton, against which no such objection is brought. There can be no reason to presume the brass work would be more defective, unless it were more complex in its form. And it is obviously far superior in point of simplicity to Cuthbertson's, which the Encyclopedia considers as " the most perfect that has yet appeared."

but less so than others.

After these strictures on the Encyclopedia Britannica, Dr. Prince gives an account of the improvements he has made on his former attempt.

Subsequent improvements.

The following improvements have been made in the American air-pump, by the inventor, to render it more simple and convenient. It has been observed above, that in all air-pumps, made to condense as well as exhaust by the same barrels and winch, there must be additional pipes, ducts and cocks to command and regulate the operations : But this is not the best method of constructing the instrument for exhausting and condensing experiments : for a great strain is brought upon the rackwork of the pump when several atmospheres are throw into the receiver : and the pump may be made with less trouble and expense by fixing a common condensing

denfing fyringe to it, in the following manner. Let a ftraight pipe be fixed to the cifterns, and pafs horizontally to the receiver-plate, as in the common table air-pump. At a convenient diftance from the barrels this pipe muft be fwelled out fo as to admit the key of a flop-cock. The key of this cock muft be pierced quite through in the direktion of its handle; and half way through, at a right angle to meet the other hole. A fmall pin muft be fixed in the handle, on that fide which correponds with the fhort hole. A hole muft be made in the fide of the pipe to correpond occasionally with the holes in the key. This cock is more fimple than the one in the original pump, and will regulate the exhaufting and condenfing experiments. To fet the cock for exhaufting the receiver, bring the handle of the key parallel with the pipe, with the folid part of the key againft the hole in the fide of the pipe; then will the communication be opened between the barrels and receiver, and the receiver may be exhaufted. To reftore the equilibrium, or let the air into the receiver, fet the handle of the key at right angles with the pipe, and let its projefting pin point to the receiver; then will the communication be opened between the atmofphere and receiver, through the hole in the fide of the pipe and the cock. In this fituation the folid part of the key will clofe the paffage in the pipe leading to the barrels. If a condenfer, having a valve at its end, be now attached to the fide of the pipe, oppofite the hole, the air may be forced into the receiver through the cock without entering the barrels. The fwelled part of the pipe, in which the key is inferted, fhould be fo made as that the condenfer may be fcrewed on or off, at pleafure. The equilibrium may be reftored in the receiver, either by unfcrewing the condenfer a little, or by letting the air out through the barrels.

Beft made to  
condenfe by  
means of a  
fyringe.

Stop-cock for  
regulating the  
experiments.

Mode of con-  
denfing.

\* In this conftruftion, the pipe ftanding between the barrels in the original pump, and the drilled paffages in the horizontal piece connecting this pipe with the regulating cock, are unnecessary. The pump is rendered more fimple, and every difficulty of execution on account of crooked paffages, &c. removed. This alteration in the American air-pump was contrived by its inventor, and a table-pump made on this plan, for him, by the late Mr. G. Adams, before the laft edition of the Encyclopedia was printed.

The pump thus  
rendered more  
fimple.

\* Another

**Alteration in  
the valve-pump.**

' Another alteration, since made, is in the situation of the valve-pump: the last mentioned pump not having one fixed to it. In all air-pumps having the tops of the barrels closed with plates and collars of leather, as in Nairne's, Cuthbertson's, and the American pump (as now altered by removing the middle pipe,) it is necessary to connect oil boxes with the top-plates to receive the oil which is thrown out of the barrels in working the pump. Cuthbertson's pump has two, one to each barrel. By removing the pipe from between the barrels, in the American pump, a small barrel is screwed in its place to the cross-piece, which connects the top-plates covering the

**Barrel to answer  
the purpose of an  
oil-box, and also  
a valve-pump.**

valves. The barrel answers the purpose of an oil-box in common exhaustions. When greater vacuums are wanted in the receiver, this barrel answers also for a valve-pump. On the top of the cross-piece is screwed a collar of leathers containing a piston and its rod, to work occasionally in the barrel below. At the lower end of the barrel is a valve covered with a cap: by unscrewing the cap, and passing down the piston, all the oil in the barrel is expelled through the valve; and afterwards the barrel, and the space above the valves on the top-plates of the great barrels, are exhausted of air, by working this small pump. The small piston when drawn up to its collar of leathers is above the holes in the cross-piece leading from the valves. When the small barrel is used only as an oil-box, the collar of leathers, with the piston, is removed, and a button, with a short pipe in it, screwed in its place to give vent to the air when expelled from the barrels: In this valve-pump there is not so much work as in Cuthbertson's two oil-boxes; nor is it an additional expence; for the syringe, which is used with the lead weight in the receiver, is made to screw to the cross-piece for this purpose; the weight being taken off, and a cap screwed on over the valve, when used as an oil-box. In the collars of leathers, on the tops of the barrels, are put two small flat boxes, below one or two rings of the leathers, the piston rods passing through them. These boxes contain the oil to keep the leathers moist, and air-tight. In this situation the oil is not thickened by evaporation, nor carried up from off the leathers, when the piston rises, as in Nairne's pump, and the leathers are better supplied than by the dirty oil passing through the pump and returned to the collars by Cuthbertson's crooked pipes. The American air-pump, made in this manner, is the simplest form of any pump of equal power.'

**This no additional  
expence.**

**The oil neither  
thickened by  
evaporation, nor  
carried off from  
the leathers.**

in this manner, is the simplest form of any pump of equal power.'

*M em air*

## V.

*Memoir on the Tides. By Cit. LAPLACE\*.*

THE object of this paper is to compare the high tides observed on the 23d of March last, with the results indicated by the theory of universal gravitation.

At this period the moon was new, and in her perigeeum. New moon, in perigeeum, and equinoctial syzgy, most favourable to high tides. These circumstances, joined to those of an equinoctial syzgy, are the most favourable to high tides: and if at such a time the action of the winds should combine with these regular causes, inundations may follow, against which it is prudent to use precautions. It is with this view that the board of longitude publishes in the *Connoissance des Temps* every year, a table of the highest tides that follow every new and full moon.

To know the real height of the tide produced by the action of the sun and moon, and distinguish it from that which is owing to the temporary action of the wind, it is not sufficient to observe the absolute height at flood, but the correspondent ebb must be observed likewise, and the difference between them gives the total height of the tide. Height of tide the difference between ebb and flood. We can easily conceive, Wind acts equally on both. that the action of the wind must add to the height of the water, both at ebb and flood, nearly in an equal degree. This consideration is indispensable, for without it all we can deduce from observation is the sum of the combined effects, without being able to separate and refer them to their real causes.

The tides of the 23d of March were observed at Brest by Cit. Rochon and Mignon. Tide of 23d of March. The total height was 23 feet four inches, the greatest ever observed. That which came nearest to it, was as far back as the 23d of September 1714, Sept. 23, 1714. when the moon was full, in her perigeeum, and almost without declination, as well as the sun. Its total height was 22 feet 11 inches.

According to the theory given in the fourth book of *La Mécanique Céleste*, the greatest difference between high and low water in the preceding syzygies, is 22 feet 10 inches, which differs very little from the observations: But in that book it is remarked, that the local circumstances of every harbour may occasion the relation of the action of the sun and moon on the

\* National Institute of France, IV.

phenomena of the tides to vary. A comparison of the observations made at Brest, has made known to Cit. Laplace, that circumstances there increase the action of the moon one-sixth; and with this modification the result of the theory is a mean between those given by observation.

Time of high-water at Brest has not varied in near a century.

The high tide of the 15th of September 1715, in the morning, and that of the 23d of March last, in the evening, were nearly equidistant from the syzygy, which should give the same hour for the tides, if the local circumstances of the harbour have not varied in the interval of nearly a century. The first was observed at half after four in the morning, true time; the second, at 29 minutes after four in the evening: whence it appears, that the time of the tides at Brest has not varied in that period.

Series of observations on the tides proposed.

Cit. Laplace has proposed to the first class of the Institute, to solicit government to direct a series of observations to be made on the tides in the different harbours of France; and to appoint a committee to draw up a single body of instructions for the best mode of making these observations. Both proposals were adopted.

The whole of the paper, of which an abstract is here given, will be printed in the *Connoissance des Temps*.

## VI.

*Abstract of a Paper by Cit. GUYTON-MORVEAU, entitled an Examination of a native Carbonate of Magnesia\*.*

Magnesia with carbonate generally in small quantity in stones.

**T**HOUGH magnesia is a constituent part of many stones, it enters into them but in small quantity, with few exceptions. Native carbonate of magnesia occurs still more rarely in any considerable proportion. Citizen Guyton, however, searching for a clay possessed of the hygrometric property in the highest degree, lately found a very large proportion of native carbonate of magnesia in a stone in the vicinity of Castellamonte, which is there considered as a clay very rich in alumine.

Characters of the stone of Castellamonte.

This stone is as compact as the hardest chalk, in an amorphous mass, and as white as ceruse. It does not sensibly adhere to the tongue, and has no argillaceous smell. Water acts

\* Bulletin des Sciences, No. 75.

very slightly on it; it is not reducible to a solid paste; yet on drying it appears to agglutinate, and contract a little its dimensions. Its specific gravity, when all the bubbles of air it contains have escaped, is 2.612. In the fire it lost 0.585 of its weight, and became sufficiently hard to scratch Bohemian glass slightly. Five grammes, being dissolved in nitric acid, gave out a large quantity of gas, by which the weight was diminished 230 centigrammes.

Concentrated sulphuric acid, poured on the stone reduced to powder, occasioned a violent effervescence on the addition of water. Without this addition the effect was not perceptible. Effervesces with sulphuric acid,

By this treatment a salt crystallized in small needles was obtained, which displayed all the properties of sulphate of magnesia. and yields sulphate of magnesia.

This salt was precipitated by potash, and the precipitate, when dried, weighed 131.5 centigrammes.

The portion not dissolved by sulphuric acid was pure silice, and weighed 71.2 centigrammes. Contains silice.

Prussiat of soda gave the solutions a slight greenish tinge, but nothing capable of being weighed was deposited.

This stone therefore contains,

Its analysis.

Magnesia	-	-	-	26.3
Silice	-	-	-	14.2
Carbonic acid	-	-	-	46
Water	-	-	-	12
Iron	-	-	-	0
Loss	-	-	-	1.5

100.0

Cit. Guyton observes, that the difference between the proportions of the constituent substances of this stone, and those of the artificial carbonate of magnesia, arises no doubt from the circumstances in which these salts have been produced; and the other characteristics, that distinguish them, may be occasioned by the presence of the other substances found with the carbonate of magnesia in the stone of Castella-Monte. How it differs from the artificial carbonate.



## • VII.

*Curious Particulars respecting the Mountains and Volcanos, and the Effect of the late Earthquakes in South America, with Remarks on the Language and Science of the Natives, and other Subjects. By M. A. VON HUMBOLDT \*.*

Three branches  
of the Andes.

WE arrived at Quito, by crossing the snows of Quiridien and Tolima, for as the cordillera of the Andes forms three separate branches, and at Santa Fe de Bogoto, we were on the easternmost, it was necessary for us to pass the loftiest, in order to reach the coast of the Pacific ocean. We travelled on foot, and spent seventeen days in these deserts, in which are to be found no traces of their ever having been inhabited. We slept in huts made of the leaves of the heliconia, which we carried with us for the purpose. Descending the Andes to the west, there are marshes, in which you sink up to the knees. The latter part of the time we were deluged with rain; our boots rotted on our legs; and we arrived barefoot at Carthago, but enriched with a fine collection of new plants, of which I have a great number of drawings.

Huts made of  
the leaves of he-  
liconia.

The Andes  
marshy toward  
the west.

Mines of Platina  
in mount Choca.

From Carthago we went to Popayan, by way of Buga, crossing the beautiful vale of the river Cauca, and having constantly at one side the mountain of Choca, in which are the mines of Platina.

Basaltic moun-  
tains.

Volcano of  
Purace.

Columnar por-  
phyritic gra-  
nites.

Paramos piercing  
cold and desti-  
tute of vegeta-  
tion.

We staid during the month of November 1801, at Popayan, visiting the Basaltic mountains of Julufuito; the mouths of the volcano of Purace, which evolve, with a dreadful noise, vapours of hydrosulphurated water; and the porphyritic granites of Pische, which form columns of five, six, or seven sides, similar to those I remember I saw in the Euganean mountains in Italy, which Strange has described.

In travelling from Popayan to Quito, we had to cross the *paramos* of Pasto, and this in the rainy season. Every place in the Andes, where, at the height of 3500 or 4000 yards, vegetation ceases, and the cold penetrates to the very marrow of your bones, is called a *paramo*. To avoid the heats

\* Abridged from the Magazin Encyclopédique.

of the valley of Patia; where, in a single night, a fever may be caught, that will last three or four months; we passed the summit of the Cordillera; traversing frightful precipices.

We spent our Christmas at Paño, a little town at the foot <sup>Town of Paño</sup> of a tremendous volcano; where we were entertained with great hospitality. The roads leading to and from it are the most shocking in the world. Thick forests; between marshes, in which the mules sink up to their bellies; and gullies so deep and narrow, that we seemed entering the galleries of a mine.

The whole province of Paño, including the environs of <sup>The province a</sup> Guachucal and Tuqueres, is a frozen plain, nearly beyond the <sup>frozen plain.</sup> point where vegetation can subsist, and surrounded by volcanos and sulphur-pits, continually emitting volumes of smoke. The wretched inhabitants of these deserts have no <sup>The people live</sup> food but potatoes: and if these fail, as they did last year, they <sup>on potatoes,</sup> go to the mountains to eat the stem of a little tree, called *achupalla* (*pourretia pitcarnia*); but the bears of the Andes, <sup>and the stems of</sup> as they too feed on it, often dispute it with them. On the <sup>the pourretia pit-</sup> north of the volcano of Paño, I discovered, in the little In- <sup>carnia.</sup> dian village of Voisaco, 1900 yards above the level of the sea, a red porphyry, with base of argil, enclosing vitreous <sup>Red porphyry</sup> feldspar, and hornblende; that has all the properties of the <sup>with distinct</sup> serpentinc of the *Fichtelgebirge*. This porphyry has very <sup>poles.</sup> distinctly marked poles, but no attractive power. Near the town of Ibarra, we nearly escaped being drowned by a very sudden swell of the water, accompanied with shocks of an earthquake.

We reached Quito on the 6th of January 1802. It is a <sup>Quito.</sup> handsome city; but the sky is commonly clouded and gloomy. The neighbouring mountains exhibit little verdure, and the cold is very considerable. The great earthquake on the 4th <sup>Earthquake of</sup> of February 1797, which changed the face of the whole <sup>1797.</sup> province; and in one instant destroyed thirty-five or forty thousand persons, has so altered the temperature of the air, <sup>altered the</sup> that the thermometer is now commonly 41° to 54°, and sel- <sup>climate greatly.</sup> dom rises to 68° or 70°, whereas Bouguer observed it constantly at 66° or 68°. Since this catastrophe, earthquakes are continually recurring; and such shocks! it is probable, that all the higher ground is one vast volcano. What are <sup>The heights one</sup> called the mountains of Cotopoxi and Pichincha, are but little <sup>vast volcano.</sup>

- summits, the craters of which, form different conduits terminating in the same cavity. The earthquake of 1797, afforded a melancholy proof of this; for the ground then opened every where, and vomited forth sulphur, water, &c. Notwithstanding the dangers and horrors that surround them, the people of Quito are gay, lively, and sociable, and in no place did I ever see a more decided and general taste for pleasure, luxury, and amusement. Thus man accustoms himself to sleep tranquilly on the brink of a precipice.
- Pichincha.** I was twice at the mouth of the crater of Pichincha, the mountain that overlooks the city of Quito. I know of no one but Condamine, that ever reached it before; and he was without instruments, and could not stay above a quarter of an hour, on account of the extreme cold. I was more successful.
- Its crater.** From the edge of the crater rise three peaks, which are free from snow, as it is continually melted by the ascending vapour. At the summit of one of these I found a rock, that projected over the precipice, and hence I made my observations. This rock was about twelve feet long, by six broad, and strongly agitated by the frequent shocks, of which we counted eighteen in less than half an hour. We lay on our bellies, the better to examine the bottom of the crater. The mouth of the volcano forms a circular hole, near a league in circumference, the perpendicular edges of which are covered with snow on the top. The inside is of a deep black; but the abyss is so vast, that the summits of several mountains may be distinguished in it. Their tops seemed to be six hundred yards below us, judge then where their bases must be. I have no doubt but the bottom of the crater is on a level with the city of Quito. Condamine found it extinct, and even covered with snow; but we had to report the unpleasant news, that it was burning. On my second visit, being better furnished with instruments, I found the diameter of the crater to be 1600 yards, whereas that of Vesuvius is but 670. The height of the mountain is 5280 yards.
- Several mountains within it.**
- Its diameter** 1600 yards;  
**height** 5280
- Volcano of Antifana, 5915 yards.** When we visited the volcano of Antifana, the weather was so favourable, that we reached the height of 5915 yards. In this lofty region, the barometer sunk to 14 inches 7 lines, [15.6 Eng.] and the tenuity of the air occasioned the blood to issue from our lips, gums, and even eyes: we felt extremely feeble, and one of our company fainted away. The air
- Barometer at** 15.6 inches.  
**Hemorrhages** brought on.

air brought from the loftiest point we visited, gave on-Air 0.218 of being analysed 0.218 of oxygen gas, and 0.008 of carbonic oxygen. acid.

We visited Cotopoxi, but could not reach the mouth of the crater. The assertion, that this mountain was diminished in height by the earthquake of 1797, is a mistake. Cotopoxi not sunk by the earthquake of 1797.

In June we proceeded to measure Chimboraco and Tunguragua, and take a plan of all the country affected by the grand catastrophe of 1797. We approached within about 500 yards of the summit of Chimboraco, our ascent being facilitated by a line of volcanic rocks bare of snow. The height we reached was 6465 yards; and we were prevented from ascending farther by a chasm too deep to cross. We felt the same inconveniences as on Antisana; and were unwell for two or three days after. The air at this height contained 0.20 of oxygen. The trigonometrical measurement I took of the mountain at two different times, and I can place some confidence in my operations, gave me for its height 6970 yards, a hundred more than Condamine assigns it. The whole of this huge mass, as of all the high mountains of the Andes, is not granite, but porphyry, from the foot to the summit, and there the porphyry is 4050 yards thick. Air at 6465 yards, contained 0.20 of oxygen. Chimboraco 6970 yards high. Consists of porphyry.

Chimboraco is probably a volcanic mountain, for the track by which we ascended, consists of a burnt and scorified rock mixed with pumice-stone, resembling all the streams of lava in this country, and ran higher up the mountain than we could climb. The summit therefore is in all likelihood the crater of an extinct volcano. Chimboraco a volcano.

The mountain of Tunguragua has diminished in height since the earthquake of 1797. Bouguer assigns it 5589 yards, I found it but 5399, so that it must have lost 190 yards; and indeed the people in the vicinity say, that they have seen its summit crumble away before their eyes. Tunguragua diminished in height. Now 5399 yards.

During our stay at Riobancha, we accidentally made a very curious discovery. The state of the province of Quito, previous to its conquest by the Inca Tupaynpangi, in 1470, is wholly unknown: but the king of the Indians, Leandro Zapla, who resides at Lican, and has a mind extraordinarily cultivated for an Indian, possesses manuscripts composed by one of his ancestors, in the sixteenth century, which contains the history of that period. They are written in the Paragay tongue, Indian manuscripts of the 16th century.

**Nevado del Atlas**, once the highest mountain in the world.

**Hieroglyphics.**

**American languages** not poor.

**Caribbee.**

**Inca.**

**Ancient science.**

**Crocodile increases air by respiration.**

**Air 274 oxygen, 15 carbonic acid, 711 azot.**

tongue, which was formerly general in Quito, but is now lost, having been supplanted by the Inca or Anichna. Fortunately another of Zapla's ancestors amused himself by translating these memoirs into Spanish. We have obtained from them valuable information, particularly in the memorable period of the eruption of Nevado del Atlas, which must have been the highest mountain in the world, loftier than Chimboraco, and called by the Indians *Capa-urcu*, or chief of mountains. These manuscripts, the traditions I collected at Parima, and the hieroglyphics I saw in the desert of Casiquiare, where scarcely a vestige of a human being is now to be seen, added to what Clavigero has said of the emigration of the Mexicans toward the south, have suggested to me ideas respecting the origin of this people, which I shall pursue when I have leisure.

I have likewise paid much attention to the study of the American languages, and found what Condamine has said of their poverty to be extremely false. The Caribbee is rich, beautiful, energetic, and polished: it is not destitute of expressions for abstract ideas; and it has numerical terms sufficient for any possible combination of figures. The Inca is particularly rich in delicacy and variety of expression. The priests knew how to draw a meridian line, and observe the solstices: they had reduced the lunar to a solar year by intercalations: and the savages even at Erevato, in the interior of Parima, believe the moon to be inhabited, and know, from the traditions of their ancestors, that its light is derived from the sun.

At Monpox I made some very curious experiments on the respiration of the crocodile, having procured forty or fifty young ones. Instead of diminishing the quantity of the air in which it respire like other animals, the crocodile increases it. A crocodile placed in 1000 parts of atmospheric air, consisting of 274 oxygen, 15 carbonic acid, and 711 azot, increased it in an hour and forty-three minutes, by the addition of 124 parts. The carbonic acid had received an augmentation of 64 parts: the oxygen had been diminished 167; but, as 46 are contained in the carbonic acid produced, the crocodile had appropriated to itself only 121 parts, a small quantity considering the colour of its blood: and 227 parts of azot, or other gasses, on which acidifiable bases

bases had no action, were produced. For the analysis I used lime water and nitrous gas, prepared with great care.

Near Santa Fee, at an elevation of 2890 yards, are found an immense number of fossil bones of elephants, both of the African species and of the carnivorous kind, discovered in North America. I have since received others from a part of the Andes, about the latitude of  $2^{\circ}$  from Quito, and from Chili: so that these animals must have existed from Patagonia to the Ohio.

Large fossil bones at a considerable height, and nearly from one extremity of America to the other.

### VIII.

*Method of measuring any Aliquot Part of an Inch by a Screw, which gives no such Part in its Turn; and Observation on an Error of Edwards in placing the Eye Stop of reflecting Telescopes. In a Letter from Mr. J. C. HORNBLLOWER.*

To Mr. NICHOLSON.

DEAR SIR,

BEING on a visit about fifteen years ago in the confines of the principality, (a region science never yet explored) I was in want of a reticulated square to be placed in the focus of the eye glass of an optical instrument, and by the disappointment usually attending jobs of this kind when done at a distance, I resolved on accomplishing it myself, though I foresaw it would be a difficult undertaking, especially as I am but an indifferent workman even with the best tools, and with such as lay before me, I could not anticipate much pleasure in my task.

Construction of a reticulated square.

The first thing which occurred to me was, the construction of the old fashioned micrometer, and I found a tap which promised well for the purpose; but unfortunately it had no determinate number of threads; but nevertheless I was resolved to proceed, and by this tap I cut a pair of dies, and by them I cut a screw on a piece of large brass wire, which being more homogenous than steel in general, had a better chance of the accuracy my instrument required, however I set the nut in a jimmel, and on trying the screw, I found it had 26,6 threads in an inch, and as I wanted tenths of an inch, I must necessarily provide means to correct the excess or defect in the screw, which I did thus:

A screw was made by tapping.

The

This was fitted up with a micrometer plate, and had a clip, by which the screw could be held fast while the plate was shifted.

The screw had a collar, which I could draw together so as to hold it fast, and another collar, on which I put the divided plate, and after that an index firmly fixed on the screw. The plate also having liberty to turn on its collar, I could fasten it independent of the screw, by having two thumb screws opposite each other, bearing on the periphery of the plate, so that I could turn the screw only, or turn the plate only, or turn them all together.

The screw was turned, the quantity answering to 0.1 inch; then clipped fast and the micrometer set to zero: the screw was set free and moved as before; then clipped, and the micrometer set; the screw then set free and moved, &c. &c.

The operation was as follows: Having laid the frame I had to divide on the board, to which this apparatus was connected, I brought the index to 0 on the plate, and made it (the plate) fast, then turned the index two revolutions and  $\frac{1}{66}$  of a revolution: This gave me a division of  $\frac{1}{66}$  inch. I then fastened the screw by bringing the collar to bind upon it; liberated the plate, and turned it until 0 on the divisions came to the index. The plate was then fastened, and the screw set free and turned 2,66 revolutions as before. The screw was then again fastened and the plate set free, and 0 brought to the index as before, after which the plate was fastened, and the screw set free and turned, &c. With a little addition of apparatus to make the fastenings and loosenings instantaneous; this method may be used to much advantage when a more elaborate or scientific instrument cannot be obtained.

I forgot in our conversation on the subject of reflecting telescopes, to bring to your mind a false calculation of Mr. Edwards's in his directions for making specula for reflecting telescopes, published in the nautical almanack some years since.

Mr. Edwards asserts that the eye stop of a reflecting telescope ought to be in the focus of the eye glass.

Concerning the place of the eye stop, his words are "It is absolutely necessary for perfect vision that the eye should be applied to a small hole of a certain dimension, placed exactly in the focus of the single eye-glass, if the eye-piece consist of one glass only, or else in the compound focus of the glasses; if it is constructed with two, as is most commonly practised;" and afterwards says, "Let the distance of the eye-hole from the eye-glass, if it is a single one, be put as near as can be attained by measure, equal to the focal distance of the eye-glass, &c."

But it ought not to be at the focus of parallel rays; but of rays from the vortex of the smaller speculum.

Surely Mr. Edwards must have taken this on a bare supposition, without ever enquiring why. When we speak of the focus of a lens, it is generally understood to mean the focus of parallel rays, but the focus of parallel rays does not determine

mine the place of the eye stop, but a focus, where the image of the small speculum is formed, and this will be more or less accordingly as the distance between the lens and the speculum is less or more. For instance, take a lens of 1,5 inch focus, which would suit a speculum of 9 inches focus, and the distance of the small speculum will be about 14 inches from the eye glass. Then  $\frac{14 \times 1,5}{14 - 1,5} = 1,68$  will be the true distance

of the eye stop, or in other words, the focus of rays proceeding from an object placed 14 inches beyond the lens, almost two inches farther than the solar focus.

I am,

DEAR SIR,

Your most obedient Servant,

J. C. HORNBLOWER.

## IX.

*Account of a new Apparatus constructed for the Purpose of measuring the Elastic Force, and regulating the Emission of Steam from the Boiler in which it is generated. Communicated by the Inventor, Mr. ARTHUR WOOLY, Engineer.*

**P**late XIV. exhibits a measured section of the self-acting Self acting and regulating steam and regulating steam valve. A A represents the upper part of the boiler, having its mouth or neck cylindrical. and closed by a well-fitted, but easily moving valve plug B B C C, which is in fact a metallic tube, open at bottom and closed above, by a cap-piece B B, that by its chamfered rim or projecting part affords the accurate valve-closure when down. The interior parallel lines at D shew the place where a long perforation is made through the side of the cylindrical part of the valve plug from its cap, nearly down to the bottom; which perforation affords a passage for the steam, increasing in magnitude as the elastic force causes the valve to rise. E is the side passage for conveying the steam to its place of operation. F is the rod or tail of the valve passing through a stuffing box above, and attached by a chain to the sector G, and by its means moving the lever that carries the ball H.

The



The above constitutes the whole of this simple and effectual contrivance, and its mode of operation scarcely needs to be described. As the steam becomes stronger it raises the valve, and escapes through D, and raises the weight H higher the more the pressure within exceeds that of the working steam in the upper space F E.

## X.

*Journey to the Summit of Mont-Perdu. By Cit. RAMOND \*.*

Sauffure's travels in the Alps highly useful to geology.

Ramond's in the Pyrenees.

Reached the summit of Mont Perdu. Vertical strata of carbonated lime. Horizontal of calcareous sandstone.

Summit a fetid lime-stone.

THE many excellent observations made by the celebrated Sauffure in the Alps, traversing that grand assemblage of mountains in all directions, have contributed more effectually to the advancement of geology, than all the hypotheses that have been formed. Cit. Ramond is rendering a similar service to the science, by his repeated journeys in the Pyrenees; and his adventurous researches will soon bring us acquainted with a great part of that chain, the structure of which is so different from that of the Alps. In a work published two years ago, he described the basis of Mont-Perdu: he had even approached its summit, and had observed that this mountain, the loftiest of the Pyrenees, was calcareous, and contained shells and other organized bodies, in a fossil state, at an elevation of about 3600 metres.

In the journey he made in August 1802, he reached the summit of the mountain by passing the Col de Fanlo, or Niscle. In this road he constantly found strata of compact carbonated lime in a position nearly vertical. They include strata of calcareous sandstone, and these strata sometimes cover the salient angles of the vertical strata, nearly in a horizontal direction. This calcareous stone falls off spontaneously in little irregular fragments: on the slightest friction it emits a nauseous fetid smell. Some of the strata of this stone contain nodules of flint; others such considerable masses of *camerines*, that the stone appears entirely composed of them. The summit is formed of a fetid lime-stone, contaminated with quartz, and containing a little iron, and  $\frac{1}{8}$  of carbon, without alumine. Cit. Ramond found no fragments of shells: but this stone being of a similar

kind to the neighbouring strata, in which they occur, he is inclined to believe, that he should discover some on a more sedulous research. The elevation of this summit is the same as Its height 3727 yards. that of the Col du Géant in the Alps, or 3426 metres.

From this, the loftiest point of the Pyrenean chain, Cit. Parallel lines of mountains on the French side Ramond could more easily observe the general form of the whole. Looking toward France, the chain is broad, and formed of several parallel lines of mountains, in the midst of which are seen the lines of granite and gneiss, of which the Granite and gneiss in the middle. peak of Bagneres is a part. These are more distinguishable by their summits being rugged with asperities, than for their elevation. These lines imperceptibly diminish in height till they reach the plain, which is too far distant to be seen. To On the Spanish side a lofty precipice : the south the appearance is very different. The whole declines suddenly, and at once. It is a precipice of ten or eleven hundred metres, the bottom of which is the summit of the highest mountains of this part of Spain. Not one of them, however, has 2500 metres absolute height, and they soon sink into low rounded hills, beyond which is the vast prospect of the plains of Arragon.

From the summit of Mont-Perdu, on the Spanish side, is Beneath this a flat of limestone, intersected by vast chasms. seen a vast flat of limestone, the surface of which, from that elevation, appears almost smooth. This flat is intersected by intersected by four or five vast chasms with perpendicular sides, the angles and sinuosities of which correspond to each other with astonishing exactness. These broad and deep chasms diverge from the base of the peak, and their bottoms are covered with thick woods. There is no way of entering them but at their mouths. Cit. Ramond proceeded by the way of Val de Broto, and entered that called by the natives Val d'Ordesa. It is a deep Val d'Ordesa. valley, uninhabited, and bordered by steep walls about 896 metres high. These you can ascend only in few places, and with the greatest difficulty. You then reach the flat. The strata that form it, and in which these vast chasms have been opened, are horizontal, or very little inclined. They consist The strata red sandstone, pudding-stone, and limestone. of red sandstone of ancient formation, pudding-stone, and compact limestone. All these stones are disposed to break off in a direction perpendicular to their beds, and Cit. Ramond ascribes this disposition to the quartz they contain. He thinks that the chasms, opened at first by some unknown cause, have been enlarged by the crumbling of their sides in this manner.

On

The peaks on the two sides rise in opposite directions.

On approaching the peaks that rise from this flat, the strata, which are of compact shelly limestone, rise at an angle of  $45^{\circ}$ , but in a direction contrary to that of the strata that form the small peaks on the northern, or French side. Thus these strata as they rise diverge like the sticks of an opened fan, the vertical ones constitute the summits; a remarkable arrangement, which Cit. Ramond ascribes to a sliding of the strata, rather than a rising up, properly so called, from a depression of the other end.

Cit. Ramond has ascertained the limits of permanent snow, and of vegetation, for this lofty part of the Pyrenean chain.

Limits of permanent snow, and of vegetation.

The snow terminates at 2440 metres. The last trees are Scotch firs, which reach 2150 metres. Next come the shrubs, of which the juniper is the highest. At 2760 metres are found the *ranunculus parnassia-folius*, the *saxifraga Groenlandica*, &c. then the *artemisia rupestris* of Lamarck; and lastly, round the very peak of Mont-Perdu, on rocks too sloping to retain the snow, grow the *cerastium*, perhaps the *alpinum* of Linneus, and the rose-flowering *aretia alpina*.

## XI.

*Notice of a Method of giving the Appearance of Cotton to Hemp or Flax\*. By Cit. BERTHOLLET.*

The author's former experi-

WHILE I was engaged in the application of the oxigenated muriatic acid, to the art of bleaching, I made experiments on flax, and I inserted this observation in the first volume of *Elemens de Tainture*, p. 258. "I have endeavoured to bleach flax completely, by the method I make use of with thread; but although its filaments may not lose much of their solidity by this, they nevertheless acquire such a tendency to separate and divide themselves, as renders them much more difficult to spin, and they form a thread of much less solidity."

Subsequent attempts.

Since that period different artists have employed themselves, with various success, in methods of obtaining from flax a matter analogous to cotton. A Swift, Cit. Clays has even formed an establishment some time ago, in which this kind of preparation is executed.

\* From Journal de L'Ecole Polytechnique, Tome IV. p. 319.

I know

I know not the processes which have been heretofore employed, but I have succeeded, by means of the oxygenated muriatic acid, in obtaining a matter more beautiful than any of those, the knowledge of which has reached me.

The very simple process I am about to describe, was executed in the laboratory of the Polytechnic School, by Cit. Gai-Lussac, at that time one of its pupils.

The flax is cut into fragments about six centimetres long; Berthollet's it is covered with water in which it is left for three or four days; process. after this it is boiled in simple water; it is washed with care; it is dyed, it is put into oxygenated muriatic acid. Four immersions in the oxygenated muriatic acid and four lyes are commonly sufficient: the operation is finished by immersing it in a bath of water charged with one two-hundredth part of sulphuric acid. On removing it from this tepid bath in which it has been left for near half an hour, it is washed with great care, and plunged into water charged with soap: it is then spread, without being wrung, on hurdles, where it is left to dry, without, however, suffering it to become too dry. All these operations, from the first immersion to the drying, do not require more than five or six hours, when the process is made with small quantities.

The flax thus prepared was then sent to Cit. Molar, who Mechanical was kind enough to undertake the mechanical operations: he operations. first combed and then carded the bleached flax. He experienced some difficulties from the knots that were scattered through the flax, but this skilful mechanic soon overcame this inconvenience. I presented to the class of physical and mathematical sciences of the Institute, on the 6th Prairial of the year 8, a sample of the prepared materials, which was equal to cotton in its whiteness, and other apparent qualities; nevertheless, Cit. Molar found fault with the cottony matter The filaments for being too short in the staple. are too short.

Cit. Bawens also manufactured the cottony matter prepared in the laboratory of the school, with the beautiful machines which he possesses at his manufactory at Chaillot. He found no difficulty in the execution, but he also found the filaments too short, although he procured a very fine thread of considerable tenacity.

It is therefore the inconvenience of being reduced into Proposed re- short filaments which requires to be corrected in the first medy. preparation;

preparation; and I am of opinion one certain method of accomplishing it, is not to complete the bleaching, but to stop at the third operation. If four are required for the thorough bleaching, it must then be finished in the thread or in the stuff.

Directions for  
the bleaching.

In the operation of bleaching, too strong lyes must be avoided, but they must be made use of in boiling. We are convinced that all the means which diminish the odour of the oxygenated muriatic acid weaken its action; hence it must be employed in a state of purity, and we must not attempt to preserve ourselves from its odour, but by the construction of the apparatus and the mode of application, objects which use has rendered easy; it must even be used in its concentrated state, otherwise the operations require to be much increased.

The process was finished by immersion in water charged with soap, which was not pressed out, in order that the filaments might not adhere too much by drying, but yield easily the separation which is to be performed by the card. But there is a probability that by preventing too much drying, the inconvenience experienced in the first trials would not take place, and that this immersion might then be omitted.

The cottony  
matter is obtain-  
ed equally good  
from fine flax or  
coarse hemp.

It is remarkable that whether the finest flax or the coarsest hemp is made use of, the filaments obtained are of equal fineness and colour.

This indication will be guide enough to artists; well acquainted with chemical manipulations, in the operation of bleaching. But I have nothing to say on the mechanical dispositions of carding and spinning, because they were not executed by me.

Probable advan-  
tages to be ob-  
tained by the  
process.

If I am not deceived, this application of a process already old, offers many advantages, because it may change the fabrication of thread, which, to this day, requires the spinning-wheel into that much less expensive; which is executed by means of machinery; and it may convert a rough product of our agriculture, and even some of the refuse, such as that from rope-walks, into a substance valuable in the arts. This motive has induced me to insert this notice in the Journal of an establishment devoted to public utility, although it offers nothing new as a scientific subject.

## XII.

*Description of a Machine now in actual and daily Use, for cleaning Chimnies, without the Assistance of Climbing-boys, and with much greater Effect than is produced by that Method. Communicated by the Inventor, Mr. CHRISTOPHER SMART, of Ordnance Wharf, Westminster Bridge. W. N.*

EVERY humane person must have beheld with pain and regret, the infant victims of a filthy and disgusting operation, who are exposed to daily suffering, and too often to permanent disease and decrepitude, without the hope of subsistence when grown to manhood. A remedy for the case of these unhappy and devoted children was long ago attempted by the amiable and benevolent Jonas Hanway; and within the last twelve months, another philanthropist, James Hebden, esq; has actively exerted himself to form a society for promoting and establishing methods of cleaning chimnies more worthy of a great and civilized people, than one grounded on the misery of the weak and the helpless. My congratulations, and those of every good man, will form but a small portion of that reward which the internal consciousness of the extensive good he has done must afford. I shall therefore dismiss any farther consideration of the personal merits of those by whom the attention of mechanics has been directed to this object, or of the artists who have laboured to solve the problem offered to their ingenuity; and shall proceed to describe Mr. Smart's machine. This apparatus has been approved upon trial by the society lately established, and has answered to the satisfaction of several well qualified employers, whose certificates I have seen, by bringing down more than the usual quantity of soot, as well as by its efficacy in lofty or winding chimnies, and such as are too narrow to be swept by children. It may easily be inferred, that it must be still more advantageous in chimnies on fire, than the shocking process of sending up a child wrapped in rags to enter an actual place of combustion and suffocating vapour.

Plate XIII. Fig. 1. represents an apparatus of brushes, suppose four, which are fixed by hinges to a middle piece or bar,

to

Machine for  
clearing chim-  
neys.

so that they shall be capable either of hanging down, parallel to the bar, or of being opened and expanded, somewhat in the manner of an umbrella, until they stand out at right angles with the middle piece; in which situation they are retained by small supporting bars, resembling those of the same well-known utensil. *Fig. 2.* shews the brushes in their collapsed state, with an appendage of tubes, by which the system is thrust up the chimney. A strong cord is passed through a series of these tubes, the lower mouth of every one of which is opened a little, in order to admit the upper ends of each in succession. *Fig. 5.* shews the sweeping man employed, thrusting the apparatus up the chimney; in which the set of connected tubes forms a piece, having enough of flexibility to accommodate itself to the chimney, and yet sufficiently rigid to answer the purpose of carrying up the set of brushes. When these have passed through the chimney-pot, and given the usual evidence of the work being to be performed from one end of the chimney to the other, the rope is drawn tight in order to set the tubes steadily together, and then secured by a thumb screw seen in *Fig. 2.*—after which, the sweeper begins to pull it downwards. The rim of the narrow opening of the chimney-pot causes the brushes to expand, and in this state they are retained by the usual spring-catch seen in *Fig. 1.* and by the simple and gradual descent, the chimney becomes effectually cleared of its soot.

*Fig. 3.* represents a curtain for defending the apartment against the soot. It is supported by a rod of metal, having a cork fixed or stuck in one of its ends, to afford a springy and perfectly harmless bearing against the inside lining of the chimney-piece, whether of marble or any other material, and the clamp which is seen towards the other end of the rod, has likewise a facing of cork, and is fixed at any distance, so as to afford the opposite bearing. The sides of the curtain are secured by rods, *Fig. 4.* which can be lengthened or shortened by two parts sliding together, as is seen in the measuring rule of shoemakers, or perhaps more familiarly, in those sliding pencils, which have now been several years in use.

I have been assured that the cleanliness, decency, and quiet operation of this engine, are by no means among the smallest of its recommendations. I have not yet had an occasion of trying it in my own house, but shall certainly do it

on the first opportunity, and shall then either confirm or modify in a future notice, as my own observation shall direct, these particulars of information, which upon good grounds I have thought myself justified in now laying before my readers.

W. N.

### XIII.

*Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat.*  
By JOHN DALTON \*.

THE progress of philosophical knowledge is advanced by the discovery of new and important facts; but much more when those facts lead to the establishment of *general laws*. It is of importance to understand that the descent of falling bodies is the same every where on the surface of the earth; but from that and some other particular facts to infer the law of gravitation, or that all matter attracts with a force decreasing as the square of the distance, is a much higher attainment in science. In the train of experiments lately engaging my attention some new facts have been ascertained, which with others, seem to authorize the deduction of general laws, and such as will have influence in various departments of natural philosophy and chemistry.

The discovery of general laws is of high value in the advancement of science.

\* These interesting treatises were read before the literary and philosophical Society at Manchester, in October, 1801, and are published in the fifth volume of their memoirs. The first, on mixed gases, was communicated in the same month, in a somewhat different form, by the author to our Journal, and published in the quarto series, vol. V. p. 241.—and, a further communication from him on the same subject appeared in vol. III. p. 267 of our present series. I have not, therefore, reprinted that essay of the present collection. The last essay in the title, viz. on the expansion of gases, is inserted in the last mentioned volume, p. 130.—Consequently though the title and introduction refer to the whole four; yet the present article contains only what was wanting to complete the readers possession of this valuable mass of experimental knowledge; that is to say, the essays upon steam and upon evaporation.



Statement of  
certain laws  
previous to their  
fundamental ex-  
periments.

As the detail of experiments will be best understood and their application seen, if the laws of principles alluded to be kept in view, it may be proper here to state them; though it must not be understood that they were proceeded upon hypothetically in the direction of those experiments. On the contrary, the first law, which is as a mirror in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained.

1. Mixed elastic  
fluids do not  
repel each other.

1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

a. The steam of  
any liquid, at  
any given num-  
ber of degrees  
from its boiling  
point, acts the  
same as that of  
any other liquid  
at the same  
number of  
degrees, the  
same way from  
its boiling point.

2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air: and that force is the same under any pressure of an other elastic fluid as it is in vacuo. Thus, the force of aqueous vapour of  $212^{\circ}$  is equal to 30 inches of mercury; at  $30^{\circ}$  below, or  $182^{\circ}$ , it is of half that force; and at  $40^{\circ}$  above, or  $252^{\circ}$ , it is of double the force; so likewise the vapour from sulphuric ether which boils at  $102^{\circ}$ , then supporting 30 inches of mercury, at  $30^{\circ}$  below that temperature it has half the force, and at  $40^{\circ}$  above it, double the force; and so in other liquids. Moreover, the force of aqueous vapour of  $60^{\circ}$  is nearly equal to half inch of mercury, when admitted into a torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

3. Evaporation  
at any tempera-  
ture is as the  
force of the  
steam.

3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

4. All elastic  
fluids expand  
equally by heat.

4. All elastic fluids expand the same quantity by heat: and this expansion is very nearly in the same equable way as that of mercury; at least from  $32^{\circ}$  to  $212^{\circ}$ .—It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation.

Having

Having now stated the chief principles which seem to be established from the following series of facts and observations, I shall proceed to treat of them under the several heads\*.

## ESSAY II.

*On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air.*

### SECTION I.—On Vapour in Vacuo.

THE term *steam* or *vapour* is equally applied to those elastic fluids which, by cold and pressure of certain known degrees, are reduced wholly or in part into a liquid state. Such are the elastic fluids arising from water, alkohol, ether, ammonia, mercury, &c. Other elastic fluids that cannot be reduced, or rather that have not yet been reduced, into a liquid state by the united agency of those two powers, are commonly denominated *gases*. There can scarcely be a doubt entertained respecting the reducibility of all elastic fluids of whatever kind into liquids; and we ought not to despair of effecting it in low temperatures and by strong pressure exerted upon the unmixed gases. However unessential the distinction between the gases and vapours may be in a chemical sense, their mechanical action is very different. By increasing the quantity of any gas in a given space the force of it is proportionally increased; but increasing the quantity of any liquid in a given space does not at all affect the force of the vapour arising from it. On the other hand, by increasing the temperature of any gas a proportionate increase of elasticity ensues; but when the temperature of a liquid is increased, the force of vapour from it is increased with amazing rapidity, the increments of elasticity forming a kind of geometrical progression, to the arithmetical increments of heat.—Thus, the ratio of the elastic force of atmospheric air of 32° to that at 212°, is nearly as 5 : 7; but the ratio of the force of aqueous vapour proceeding from water of 32° and 212°, is as 1 : 150 nearly.

The object of the present essay is to determine the utmost force that certain vapours, as that from water, can exert at different temperatures. The importance hitherto attached to this enquiry has arisen chiefly from the consideration of steam

*Steam or vapour defined. It is an elastic fluid capable of becoming liquid by cold and pressure. Gases not so.*

*Remarkable difference between the expansion of steam and of gas by heat: the former being prodigiously greater.*

*Object of the present essay.*

\* Here followed the essay I. on mixed gases. W. N.

Reference to  
authors respect-  
ing steam.

Encyl. Britt.  
Betancourt.

The author's  
method. A  
minute portion  
of the fluid is  
put into the  
upper space of a  
barometer. Heat  
is applied by the  
external contact  
of water. The  
fall of the mer-  
cury shews the  
effect.

as a mechanical agent; and this has directed the attention more especially to high temperatures. But it will appear from what follows that the progress of philosophy is more immediately interested in accurate observations on the force of steam in low temperatures. Different authors have published accounts of their experiments on the force of steam: I have on a former occasion (*Meteorological Essays*, page 134) given a table of forces for every 10° from 80° to 212°. The author of the article "Steam," in the *Encyclopedia Britannica*, has done the same from 32° to 280°: and M. Betancourt, in the "*Memoirs des savans etrangeres*" for 1790, see Hutton's *Math. Diction.* page 755) has given tables on the subject, both for vapour from water and spirit of wine, also from 32° to 280°. But these two authors, having assumed the force of vapour from water of 32° to be nothing, are essentially wrong at that point and in all the lower parts of the scale; and in the higher part, or that above 212°, they determine the force too much: owing as I apprehend to a quantity of air, which being disengaged from the water by heat and mixing with the steam, increases the elasticity.—In a question of such moment it seemed therefore desirable to obtain greater accuracy.

My method is this: I take a barometer tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air: the barometer by standing some time exhibits a portion of water, &c. of  $\frac{1}{4}$  or  $\frac{1}{16}$  of an inch upon the top of the mercurial column; because being lighter it ascends by the side of the tube; which may now be inclined and the mercury will rise to the top manifesting a perfect vacuum from air. I next take a cylindrical glass tube open at both ends, of 2 inches diameter and 14 inches in length; to each end of which a cork is adapted, perforated in the middle so as to admit the barometer tube to be pushed through and to be held fast by them; the upper cork is fixed two or three inches below the top of the tube and is half cut away so as to admit water, &c. to pass by; its service being merely to keep the tube steady. Things being thus circum-

stanced

stanced, water of any temperature may be poured into the wide tube, and thus made to surround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be observed from the depression of the mercurial column. In this way I have had water as high as  $155^{\circ}$  surrounding the vacuum: but as the higher temperatures might endanger a glass apparatus; instead of it I used the following:—

Having procured a tin tube of four inches in diameter and two feet long, with a circular plate of the same soldered to one end having a round tube in the center like the tube of a reflecting telescope, I got another smaller tube of the same length soldered into the larger, so as to be in the axis or centre of it: the small tube was open at both ends, and on this construction water could be poured into the large vessel to fill it, whilst the central tube was exposed to its temperature. Into this central tube I could insert the upper half of a syphon barometer, and fix it by a cork, the top of the narrow tube also being corked; thus the effect of any temperature under  $212^{\circ}$  could be ascertained, the depression of the mercurial column being known by the ascent in the exterior leg of the syphon.

The force of vapour from water between  $80^{\circ}$  and  $212^{\circ}$  may also be determined by means of an air-pump; and the results exactly agree with those determined as above. Take a Florence flask half filled with hot water, into which insert the bulb of a thermometer; then cover the whole with a receiver on one of the pump-plates, and place a barometer gage on the other: the air being slowly exhausted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the observed temperature. This method may also be used for other liquids. It may be proper to observe the various thermometers used in these experiments were duly adjusted to a good standard one.

After repeated experiments by all these methods, and a careful comparison of the results, I was enabled to digest the following table of the force of steam from water in all the temperatures from  $32^{\circ}$  to  $212^{\circ}$ .

Two important enquiries still remained, the first to determine the force of steam from water above  $212^{\circ}$  and below

The vessel containing the external water was of tin for temperatures near boiling; and a syphon barometer was used to shew the depression.

Another method by observing the boiling point in the air pump, and the station of the barometer gage.

Hence the force of steam up to  $212^{\circ}$  was bad.

32°; the second, to determine the comparative forces of vapour from other liquids. These enquiries seemed independent of each other; notwithstanding which I found them in reality connected.

Examination of  
the progression  
of the force of  
vapour.

Upon examination of the numbers in the table, within the limits just mentioned, there appears something like a geometrical progression in the forces of vapour; the ratio, however, instead of being constant, is a gradually diminishing one: thus the

Force at 32° =	,200 inch.	
	17. 50	} Ratios.
122 =	3. 500	
212 =	30. 000	

If we divide these ratios, according to observation, they will stand thus:

Force at 32° =	,200 inch.	
	4. 550	} Ratios.
77 =	,910	
122 =	3. 500	
	3. 214	
167 =	11. 250	
212 =	30. 000	

If we divide these again, they become,

Force at 32° =	,200 inch.	
	2. 17	} Ratios.
54½ =	,435	
	2. 09	
77 =	,910	
	2. 00	
99½ =	1. 820	
	1. 92	
122 =	3. 500	
	1. 84	
144½ =	6. 450	
	1. 75	
167 =	11. 250	
	1. 67	
189½ =	18. 800	
	1. 59	
212 =	30. 000	

By another division we obtain the ratios for every  $11\frac{1}{4}^{\circ}$  of temperature from  $32^{\circ}$  to  $212^{\circ}$ , as under :

of Examination of  
the progression  
of the force of  
vapour.

Force at $32^{\circ}$ =	,200 inch	
		1. 485
$43\frac{1}{4}$ =	,297	
		1. 465
$54\frac{1}{2}$ =	,435	
		1. 45
$65\frac{3}{4}$ =	,630	
		1. 44
77 =	,910	
		1. 43
$88\frac{1}{2}$ =	1. 290	
		1. 41
$99\frac{1}{2}$ =	1. 820	
		1. 40
$110\frac{1}{2}$ =	2. 510	
		1. 38
122 =	3. 500	
		1. 36
$133\frac{1}{4}$ =	4. 760	
		1. 35
$144\frac{1}{2}$ =	6. 450	
		1. 33
$155\frac{3}{4}$ =	8. 550	
		1. 32
167 =	11. 250	
		1. 30
$178\frac{1}{4}$ =	14. 600	
		1. 29
$189\frac{1}{2}$ =	18. 800	
		1. 27
$200\frac{1}{4}$ =	24. 000	
		1. 25
212 =	30. 000	

Ratios,

Thus it appears that a ratio having a uniform decrease nearly takes place; and we may therefore extend the table of forces at both extremes, without the aid of experiment, to a considerable distance. Thus assuming the ratios for each interval of a  $11\frac{1}{4}$  below  $32^{\circ}$  to be, 1.500, 1.515, 1.530, 1.545, &c. and for each interval above  $212^{\circ}$  to be 1.235, 1.220, 1.205, 1.190, 1.175, 1.160, 1.145, 1.130, &c. we can extend the table many intervals of temperature, and determine all the intermediate degrees by interpolation. This method may be relied upon as a near approximation;

The ratio of  
augmentation  
in the force of  
vapour is not as  
the degrees,  
but gradually  
left.

however it does not supersede the expediency of determination by experiment; though that is much more difficult above  $212^{\circ}$ , and below  $32^{\circ}$ , than in the intermediate degrees; because it is difficult to procure a steady heat above  $212^{\circ}$ ; and below  $32^{\circ}$  the variation of force becomes so small as to elude minute discrimination. It will appear from what follows that the extension of the table by this method above  $212^{\circ}$  is in all probability accurate, or very nearly so, for  $100^{\circ}$  or more.

## T A B L E

Table of the  
force of aqueous  
vapour or steam.

Of the Force of Vapour from Water in every temperature from that of the congelation of Mercury, or  $40^{\circ}$  below zero of Fahrenheit, to  $325^{\circ}$ .

Temper- ature.	Force of Vap- in inches of Mercury.	Temper- ature.	Force of Vap- in inches of Mercury.	Temper- ature.	Force of Vap- in inches of Mercury.
$-40^{\circ}$	,013	$25^{\circ}$	,156	$54^{\circ}$	,429
$-30$	,020	26	,162	55	,443
$-20$	,030	27	,168	56	,458
$-10$	,043	28	,174	57	,474
—	—	29	,180	58	,490
0	,064	30	,186	59	,507
1	,066	31	,193	60	,524
2	,068	—	—	61	,542
3	,071	32	,200	62	,560
4	,074	33	,207	63	,578
5	,076	34	,211	64	,597
6	,079	35	,221	65	,616
7	,082	36	,229	66	,635
8	,085	37	,237	67	,655
9	,087	38	,245	68	,676
10	,090	39	,254	69	,698
11	,093	40	,263	70	,721
12	,096	41	,273	71	,745
13	,100	42	,283	72	,770
14	,104	43	,294	73	,796
15	,108	44	,305	74	,823
16	,112	45	,316	75	,851
17	,116	46	,328	76	,880
18	,120	47	,339	77	,910
19	,124	48	,351	78	,940
20	,129	49	,363	79	,971
21	,134	50	,375	80	1. 00
22	,139	51	,388	81	1. 04
23	,144	52	,401	82	1. 07
24	,150	53	,415	83	1. 10

Tens.

Table continued.

Temper- ature.	Force of Vap- in inches of Mercury.	Temper- ature.	Force of Vap- in inches of Mercury.	Temper- ature.	Force of Vap- in inches of Mercury.	Table of the force of aqueous vapor or steam.
84°	1. 14	128°	4. 11	172°	12. 73	
85	1. 17	129	4. 22	173	13. 02	
86	1. 21	130	4. 34	174	13. 32	
87	1. 24	131	4. 47	175	13. 62	
88	1. 28	132	4. 60	176	13. 92	
89	1. 32	133	4. 73	177	14. 22	
90	1. 36	134	4. 86	178	14. 52	
91	1. 40	135	5. 00	179	14. 83	
92	1. 44	136	5. 14	180	15. 15	
93	1. 48	137	5. 29	181	15. 50	
94	1. 53	138	5. 44	182	15. 86	
95	1. 58	139	5. 59	183	16. 23	
96	1. 63	140	5. 74	184	16. 61	
97	1. 68	141	5. 90	185	17. 00	
98	1. 74	142	6. 05	186	17. 40	
99	1. 80	143	6. 21	187	17. 80	
100	1. 86	144	6. 37	188	18. 20	
101	1. 92	145	6. 53	189	18. 60	
102	1. 98	146	6. 70	190	19. 00	
103	2. 04	147	6. 87	191	19. 42	
104	2. 11	148	7. 05	192	19. 86	
105	2. 18	149	7. 23	193	20. 32	
106	2. 25	150	7. 42	194	20. 77	
107	2. 32	151	7. 61	195	21. 22	
108	2. 39	152	7. 81	196	21. 68	
109	2. 46	153	8. 01	197	22. 13	
110	2. 53	154	8. 20	198	22. 69	
111	2. 60	155	8. 40	199	23. 16	
112	2. 68	156	8. 60	200	23. 64	
113	2. 76	157	8. 81	201	24. 12	
114	2. 84	158	9. 02	202	24. 61	
115	2. 92	159	9. 24	203	25. 10	
116	3. 00	160	9. 46	204	25. 61	
117	3. 08	161	9. 68	205	26. 13	
118	3. 16	162	9. 91	206	26. 66	
119	3. 25	163	10. 15	207	27. 20	
120	3. 33	164	10. 41	208	27. 74	
121	3. 42	165	10. 68	209	28. 29	
122	3. 50	166	10. 96	210	28. 84	
123	3. 59	167	11. 25	211	29. 41	
124	3. 69	168	11. 54	212	30. 00	
125	3. 79	169	11. 83			
126	3. 89	170	12. 13	213	30. 60	
127	4. 00	171	12. 43	214	31. 21	

Tem-



Table of the  
force of aqueous  
vapor or steam.

Table continued.

Temper- ature.	Force of Vap. in inches of Mercury.	Temper- ature.	Force of Vap. in inches of Mercury.	Temper- ature.	Force of Vap. in inches of Mercury.
215°	31. 83	252°	60. 05	289°	98. 96
216	32. 46	253	61. 00	290	100. 12
217	33. 09	254	61. 92	291	101. 28
218	33. 72	255	62. 85	292	102. 45
219	34. 35	256	63. 76	293	103. 63
220	34. 99	257	64. 82	294	104. 80
221	35. 63	258	65. 78	295	105. 97
222	36. 25	259	66. 75	296	107. 14
223	36. 88	260	67. 73	297	108. 31
224	37. 53	261	68. 72	298	109. 48
225	38. 20	262	69. 72	299	110. 64
226	38. 89	263	70. 73	300	111. 81
227	39. 59	264	71. 74	301	112. 98
228	40. 30	265	72. 76	302	114. 15
229	41. 02	266	73. 77	303	115. 32
230	41. 75	267	74. 79	304	116. 50
231	42. 49	268	75. 80	305	117. 68
232	43. 24	269	76. 82	306	118. 86
233	44. 00	270	77. 85	307	120. 03
234	44. 78	271	78. 89	308	121. 20
235	45. 58	272	79. 94	309	122. 37
236	46. 39	273	80. 98	310	123. 53
237	47. 20	274	82. 01	311	124. 69
238	48. 02	275	83. 13	312	125. 85
239	48. 84	276	84. 35	313	127. 00
240	49. 67	277	85. 47	314	128. 15
241	50. 50	278	86. 50	315	129. 29
242	51. 34	279	87. 63	316	130. 43
243	52. 18	280	88. 75	317	131. 57
244	53. 03	281	89. 87	318	132. 72
245	53. 88	282	90. 99	319	133. 86
246	54. 68	283	92. 11	320	135. 00
247	55. 54	284	93. 23	321	136. 14
248	56. 42	285	94. 35	322	137. 28
249	57. 31	286	95. 48	323	138. 42
250	58. 21	287	96. 64	324	139. 56
251	59. 12	288	97. 80	325	140. 70

Vapor from  
ether and from  
other liquids  
follow a general  
law in their  
force.

#### *On Vapour from Ether, &c.*

We come now to the consideration of vapour from other liquids. Some liquids are known to be more evaporable than water; as liquid ammonia, ether, spirit of wine, &c. others less; as, quicksilver, sulphuric acid, liquid muriate of

lime,

lime, solution of potash, &c. and it appears that the force of vapour from each in a vacuum is proportionate to its evaporability. M. Betancourt maintains that the force of vapour from spirit of wine is in a constant ratio to that from water at all temperatures; namely, as 7 to 3 nearly. My first experiments with spirits of wine led me to adopt this conclusion, and naturally suggested that the force of vapour from any other liquid would bear a constant ratio to that of water. The principle, however, is not true, either with regard to spirit of wine or any other liquid. Experiments made upon six different liquids agree in establishing this as a general law; namely, *that the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force*: thus assuming a force equal to thirty inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find *aqueous* vapour loses half its force by a diminution of 30° degrees of temperature; so does the vapour of any other liquid lose half its force by diminishing its temperature thirty degrees below that in which it boils; and the like for any other increment or decrement of heat. This being the case, it becomes unnecessary to give distinct tables of the force of vapour from different liquids, as one and the same table is sufficient for all. But it will be proper to relate the experiments on which this conclusion rests.

The law enun-  
ciated.

#### *Experiment on Sulphuric Ether.*

The ether I used boiled in the open air at 102°. — I filled a barometer tube with mercury, moistened by agitation in ether. After a few minutes a portion of ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the air in the room, 62°, the mercury stood at 17.00 inches, the barometer at the same time being 29.75. Hence the force of vapour from ether at 62 is equal to 12.75 inches of mercury, which accords with the force of aqueous vapour at 172°, temperatures which are 40° from the respective boiling points of the liquids. By subsequent observations I found the forces of the vapour from ether in all the different temperatures from 33° to 102° exactly corresponded with the forces of aqueous vapour of the like range

Experiments  
with ether; in  
the barometer,  
below ebullition.

range, namely from  $142^{\circ}$  to  $212^{\circ}$ : the vapour from ether depresses the mercury about six inches in the temperature of  $32^{\circ}$ .

above ebullition

Finding that ether *below* the point of ebullition agreed with water below the said point, I naturally concluded that ether *above* the point would give the same force of vapour as water above it; and in this I was not disappointed; for, upon trial it appeared that what I had inferred only from analogical reasoning respecting the force of aqueous vapour above the boiling point, actually happened with that from ether above the said point. And ether is a much better subject for experiment in this case than water, because it does not require so high a temperature.

with the syphon  
barometer.

I took a barometer tube of 45 inches in length, and having sealed it hermetically at one end, bent it into a syphon shape, making the legs parallel, the one that was close being nine inches long, and the other 36. Then conveyed two or three drops of ether to the end of the closed leg, and filled the rest of the tube with mercury, except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water; the ether thus exposed to a heat above the temperature at which it boils, produced a vapour more powerful than the atmosphere, so as to overcome its pressure and raise a column of mercury besides, of greater or less length according to the temperature of the water. When the water was at  $147^{\circ}$  the vapour raised a column of 35 inches of mercury, when the atmospheric pressure was 29.75: so that vapour from ether of  $147^{\circ}$  is equivalent to a pressure of 64.75 inches of mercury; agreeing with the force of aqueous vapour of  $257^{\circ}$ , according to the preceding estimation: in both cases the temperatures are  $45^{\circ}$  above the respective points of ebullition. In all the temperatures betwixt  $102^{\circ}$  and  $147^{\circ}$  the forces of ethereal vapour corresponded with those of aqueous vapour, as per table, betwixt  $212^{\circ}$  and  $257^{\circ}$ . I could not reasonably doubt of the equality continuing in higher temperatures; but the force increases so fast with the increase of heat, that one cannot extend the experiments much farther without tubes of very inconvenient lengths. Being desirous however to determine the

Experiments in which the reduction of included air was used as the measure of force,

force

force of the ethereal vapour experimentally up as high as  $212^{\circ}$ , I contrived to effect it as follows:—Took a syphon tube such as described above, only not quite so long, and filled it in the manner above mentioned, with ether and mercury, leaving about ten inches at the top of the tube vacant; then having graduated that part into equal portions of capacity, and dried it from ether, I drew out the end of the tube to a capillary bore, cooled it again so as to suffer the internal atmospheric air to be of the proper density, and suddenly sealed the tube hermetically, thus inclosing air of a known force in the graduated portion of the tube. Then, putting that part of the tube containing ether into boiling water, vapour was formed which forced the mercurial column upwards and condensed the confined air, till at length an equilibrium took place. In this way I found 8.25 parts of atmospheric air of the force 29.5 were condensed into 2.00, at the same time a perpendicular column of 16 inches of mercury in addition pressed upon the vapour. Now the force of elastic fluids being inversely as the space, we have  $2.00 : 29.5 :: 8.25 : 121.67$  inches = the force of the air within; to which adding 16 inches, we obtain 137.67 = the whole force sustained by the vapour, measured in inches of mercury. The force of aqueous vapour, at the same distance beyond the boiling point, or  $322^{\circ}$ , is equal to 137.28, per table. Thus it appears that in every part of the scale on which experiments have been made, the same law of force is observable with the vapour of ether as of water.

#### *Experiments on Spirit of Wine.*

By boiling a small portion of the spirit I used (about one cubic inch) in a phial, the thermometer stood at  $179^{\circ}$  at the commencement; but by continuing the ebullition it acquired a greater heat. The reason is, the most evaporable part of the spirit flies off during the process of heating, and the rest being a weaker compound, requires a stronger heat. The true point of ebullition, I believe, was nearly  $175^{\circ}$ .—The force of the vapour from this spirit at the temperature of  $212^{\circ}$ , I found both by an open syphon tube and one hermetically sealed with atmospheric air upon the mercurial column, as with ether, to be equal to  $58\frac{1}{2}$  inches of mercury.

This

This rather exceeds the force of aqueous vapour at an equal distance from the boiling point; but it is no more than may be attributed to unavoidable little errors in such experiments. In a barometer tube the spirituous vapour at  $60^{\circ}$ , over the mercury, depresses the column about 1.4 or 1.5 inches; which is something less than the due proportion; one cause of this may be the evaporability of spirits, which in operating on small quantities, quickly dissipates part of their strength.

*Experiments on Liquid Ammonia.*

Force of vapor  
of liquid am-  
monia,

Liquid ammonia or volatile alkali, the specific gravity of which was .9474, boiled near  $140^{\circ}$ ; in the barometer a small quantity depressed the mercury 4.3 inches in the temperature of  $60^{\circ}$ . In higher temperatures it did not produce a proportionable depression; because the most volatile part of the compound, expanding in the vacuum of the barometer; leaves the rest more watery, and consequently its vapour must be weaker; especially when the portion used is confined to a drop or two.

*Muriate of Lime.*

of muriate of  
lime.

Put a portion of liquid muriate of lime over the column of mercury in a barometer. The boiling point of the muriate was found by experiment to be  $230^{\circ}$ . At  $55^{\circ}$  the depression was .22 of an inch:

at  $65^{\circ}$ —.30

—  $70^{\circ}$ —.40

—  $95^{\circ}$ —.90

all which nearly agree with the forces of aqueous vapour  $19^{\circ}$  below the respective temperatures.

*Mercury and Sulphuric Acid.*

Forces of the  
vapour of mer-  
cury and sul-  
phuric acid.

Mercury boils by my thermometer at  $660^{\circ}$ , and sulphuric acid of the specific gravity 1.83, boils at  $590^{\circ}$ . It is very difficult to determine the precise force of vapour from these liquids in any temperature under  $212^{\circ}$ ; because at such great distance from the boiling point the vapour is so weak as to be in effect almost imperceptible. Following the general law, the vapours of these fluids ought to be of the force .1, mercury at  $460^{\circ}$ . and sulphuric acid at  $390^{\circ}$ .—Col. Roi makes the expansion of 30 inches mercury by  $180_{\circ}$  of heat = .5969 or .5651; and in a barometer the expansion in the  
same

same circumstances is .5117; the differences are .0652 and .0534 which should measure the effective force of mercurial vapour of 212°, nearly. This is in all probability too much; as it is next to impossible to free any liquid entirely from air; and if any air enter the vacuum, it unites its force to that of the mercurial vapour.

That the force of vapour from sulphuric acid, in low temperatures, is exceedingly small, will appear from the ensuing section.

## SECTION II.

*On Vapour in Air.*

The experiments under this head were made with manometers, or straight tubes of different lengths, hermetically sealed at one end, of  $\frac{1}{12}$  inch internal diameter, and their capacities divided into equal portions. A drop or two of the liquid, the subject of experiment, was conveyed to the bottom or sealed end of the tube; the internal surface was then dried by a wire and thread, and atmospheric, (or any other air) was admitted into the tube, upon which a column of mercury was suspended of  $\frac{1}{16}$  of an inch, or of 30 inches, less or more, according to the nature of the experiment. By immersing the end of the manometer, containing the air thus circumstanced, into a tall glass vessel containing water of any temperature, the effect of the vapour in expanding the air could be perceived. It was first indeed necessary to determine the increase air unaffected by any liquid (except mercury) would obtain by increase of temperature: that was done, as will be particularly shewn in the next essay.\* The expansion of all elastic fluids, it seems probable, is alike or nearly so, in like circumstances; 1000 parts of any elastic fluid expands nearly in a uniform manner into 1370 or 1380 parts by 180° of heat.

It will be unnecessary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the results of all agree in one general rule or principle, which is this: let 1 represent the space occupied by any kind of air of a given temperature and free from moisture;  $p$  = the given pressure upon it, in inches of mercury;  $f$  = the

Effect of the expansion of vapour in air. The experiments made by a small tube, stopped by a moveable plug of mercury.

General law of expansion of air and vapour together. The space at any temperature is directly as the pressure and inversely as the pressure less the force of the force vapour.

force of vapour from any liquid in that temperature, in *vacuo*; then, the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately, or in a short time  $= 1 + \frac{f}{p-f}$ ; or, which is the same thing,

$$= \frac{p}{p-f}.$$

Thus in water for instance :

Let  $p=30$  inches,

$f=15$  inches, to the given temp.  $180^{\circ}$ .

Then,  $\frac{p}{p-f} = \frac{30}{30-15} = 2$ , for the space; or the air becomes of twice the bulk.

If the temperature be  $203^{\circ}$ ,  $f=25$ , and the space becomes six times as large as at first.

If  $p=60$  inches

$f=30$  inches to the given temperature  $212^{\circ}$ ; then the space  $= \frac{60}{60-30} = 2$ ; or water under the pressure of 60 inches of mercury, and at the temperature of  $212^{\circ}$ , produces vapour which just doubles the volume of air.

If ether be the instance: let the temperature be equal  $70^{\circ}$ ; then  $f=15$ ; and suppose  $p=30$ ; in this case the volume of air is doubled; that is, ether of  $70^{\circ}$  being admitted to any portion of air, doubles its bulk.

The expansion of hydrogenous gas and atmospheric air by the vapour of water is the same for every temperature.

Sulphuric acid does not expand atmospheric air to any sensible amount by the heat of boiling water.

The theory of these facts is evident upon the principles laid down in the former essay: for instance; let it be required to explain the experiment with water of  $212^{\circ}$  under a pressure of 60 inches. Here the air was condensed into the space 1 by the pressure of 60 inches; but being exposed to water of  $212^{\circ}$ , a vapour arose from it equal in force to 30 inches; the air therefore expanded till its force also became  $=$  to 30 inches, which was effected by doubling its volume: then the vapour pressing with 30 inches force and the air also with 30 inches force, the two together support the pressure of 60 inches and the equilibrium continues. In short, in all cases the vapour arises to a certain

certain force, according to temperature, and the air adjusts the equilibrium, by expanding or contracting as may be required.

The notion of a chemical affinity subsisting between the gases and vapours of different kinds, cannot at all be reconciled to these phenomena. To suppose that all the different gases have the same affinity for water might indeed be admitted if we could not explain the phenomena without it; but to go further, and suppose that water combines with every gas to the same amount as its vapour in vacuo; or in other words, that the elasticity of the compound should be exactly the same as if the two were separate, is certainly going far to serve an hypothesis.

*These facts do not agree with the notion of chemical affinity between gas and vapour.*

Besides, we must on this ground suppose that all the gases have the same force of affinity for any given vapour; a supposition that cannot be admitted as having any analogy to other established laws of chemical affinity.

*(To be continued.)*

#### XIV.

*Description of the Portable Furnace constructed by Dr. Black, and since improved. In a Letter from Mr. Accum.*

To Mr. NICHOLSON.

DEAR SIR,

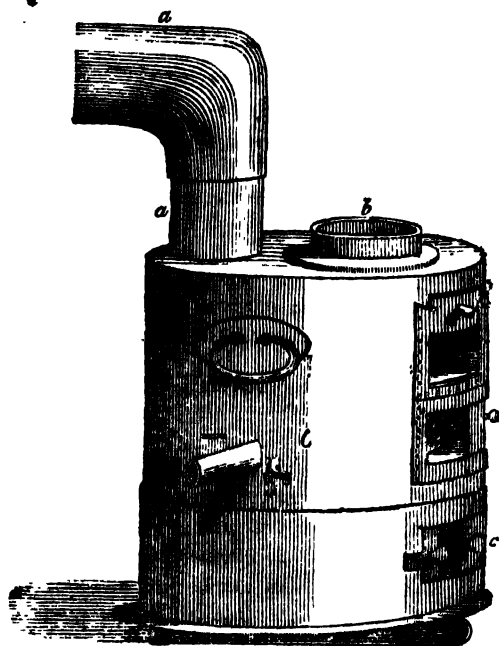
IN my System of Practical Chemistry, Vol. II. p. 357, I have given a Description and Drawing of a Portable Universal Furnace, which in the practice of my profession I found the best furnace for all chemical operations whatever which require like aid. The number of furnaces which I have caused to be made for different philosophers of that kind, and the useful hints which I have received from different quarters, have materially improved it, that I flatter myself, whether a description of this furnace would not be acceptable to your readers, particularly to those who have no access to the laboratory of the operative chemist; for those who are familiar with practical chemistry will readily allow, that a furnace capable of producing a very low and very intense heat is one of the most requisite and most indispensable instruments of all the apparatus of chemistry. The

*Description of the portable furnace in chemical operations.*



Description of  
the portable fur-  
nace in chemical  
operations.

great advantage of this furnace (*which was first invented by Dr. Black, and improved by others*) above all others I am acquainted with, consists in consuming as little fuel as possible, in producing quickly, if required, a very intense heat—in regulating expeditiously, and at pleasure, its intensity—in applying it as directly, and as fully as possible, to the substances upon which it is intended to act—and moreover in enabling the operator to perform his operations in the closet, or in any other place, without the risk of endangering the conflagration of the surrounding objects, which were not meant to be exposed to the action of heat.



This portable universal furnace is made of strong wrought iron plates. It is lined with bricks, bedded in fire-proof loam. Its height without the chimney *a a* is two feet. The inner diameter of the cylindrical fire-place measures ten inches. The body of the furnace is elliptical; in its upper part a circular hole is cut, for receiving an iron sand-pot *b* which may occasionally be removed and exchanged for an iron plate. In the front of the furnace there are three openings over each other, furnished with sliding doors, and fitted with

with stoppers made of crucible ware. The lower opening *c*, Description of the portable furnace in chemical operation. is the ash-pit of the furnace; it is composed of two register plates, sliding backwards and forwards in grooves, in order to diminish, or enlarge the opening for regulating the heat; by admitting or excluding air at pleasure. In the side of the furnace a hole is cut, furnished with a stopper and door, for passing a tube through the fire-place of the furnace; an expedient very necessary for a variety of chemical processes, such as exhibiting the decomposition of water, alcohol, oils, &c. for the preparation of phosphuret of lime, for passing gases over ignited bodies, &c. In either of the openings in front of the furnace, a muffle may be placed for performing the process of cupellation of gold; silver, &c. or, the neck of a retort (placed on a stand in the body of the furnace) may be passed through it, for distillation by the naked fire; for procuring gases which require a high degree of heat, &c. If the iron sand-pot *b* be removed, and a circular plate properly lined with fire-clay be placed in its room, the furnace becomes converted into a wind-furnace; the fuel is then to be introduced through either of the openings in front. The iron plate at the top has a hole in the centre, furnished with a stopper, to enable the operator to inspect his process at pleasure. If the iron-pot be placed inverted on the opening of the furnace; it forms a dome, and it then becomes a reverberating furnace. The iron-pot when filled with sand, or water, placed in its proper situation, serves as a sand or water-bath, for the processes of distillation by means of glass retorts, for evaporations, sublimations, digestions, &c. Coake and charcoal are the best fuel, this mixture burns without smoke, and gives a strong uniform and permanent heat; charcoal and common coal, or coal only, does likewise very well. The elbow of the chimney *a* may be directed into that of the fire-place of any apartment.

The furnace is furnished with castors, and may therefore be easily moved, according to the convenience of the operator. I am,

S I R,

Your most obedient servant,

FREDERICK ACCUM.

*Old Compton Street, Soho,*

*Nov. 21, 1803.*

T 2

XV. Observations

## XV.

*Observations on the Structure of the Tongue; illustrated by Casts in which a Portion of that Organ has been removed by Ligation. By EVERARD HOME, Esq. F. R. S.\**

## Introduction.

**PHYSIOLOGICAL** inquiries have ever been considered as deserving the attention of this learned Society; and, whenever medical practitioners, in the treatment of diseases, have met with any circumstance, which threw light upon the natural structure or actions of any of the organs of the human body, or those of other animals, their communications have met with a favourable reception.

Importance of  
a safe means of  
removing part  
of the tongue.

The following observations derive their real importance from offering a safe and effectual means of removing a portion of the tongue, when that organ has taken on a diseased action, the cure of which is not within the reach of medicine; and, as the tongue, like many other glandular structures, is liable to be affected by cancer, it becomes of no small importance that the fact should be generally known. In a physiological view, they tend to show, that the internal structure of the tongue is not of that delicate and sensible nature which, from its being the organ of taste, we should be led to imagine.

## Its structure

The tongue is made up of fasciculi of muscular fibres, with an intermediate substance met with in no other part of the body, and a vast number of small glands; it has large nerves passing through it; and the tip possesses great sensibility, fitting it for the purpose of taste,

Whether the sense of taste is confined entirely to the point of the tongue, and the other parts are made up of muscles fitted for giving it motion; or whether the whole tongue is to be considered as the organ, and the soft matter which pervades its substance, and fills the interstices between the fasciculi of muscular fibres, is to be considered as connected with sensation, has not, I believe, been ascertained.

supposed to be  
very delicate.

The tongue, throughout its substance, has always been considered by physiologists as a very delicate organ; and it was believed, that any injury committed upon it would not

\* *Philos. Transact.* 1803, p. 205.

only produce great local irritation, but also affect, in a violent degree, the general system of the body. This was my own opinion, till I met with the following case, the circumstances of which induced me to see this organ in a different point of view.

A gentleman by an accident which it is unnecessary to describe, had his tongue bitten with great violence. The immediate effect of the injury was great local pain; but it was not attended with much swelling of the tongue itself, nor any other symptom, except that the point of the tongue entirely lost its sensibility, which deprived it of the power of taste: whatever substance the patient eat was equally insipid. This alarmed him very much, and induced him to state to me the circumstances of his case, and request my opinion. I examined the tongue a fortnight after the accident. It had the natural appearance, but the tip was completely insensible, and was like a piece of board in his mouth, rendering the act of eating a very unpleasant operation. I saw him three months afterwards, and it was still in nearly the same state.

Accident of the tongue very much bitten, and rendered insensible,

From this case it appears, that the tongue itself is not particularly irritable; but the nerves passing through its substance to supply the tip, which forms the organ of taste, are very readily deprived of their natural action; this probably arises from their being softer in texture than nerves in general, and in that respect, resembling those belonging to the other organs of sense.

There was another circumstance in this case which very particularly struck my attention, viz. that a bruise upon the nerves of the tongue, sufficient to deprive them of the power of communicating sensation, was productive of no inflammation or irritation in the nervous trunk, so as to induce spasms, which too commonly occur from injuries to the nerves belonging to voluntary muscles. I am therefore led to believe, that the nerves supplying an organ of sense, are not so liable to such effects as those which belong to the other parts of the body.

without inflammation or irritation to produce spasms.

The small degree of mischief which was produced, and the readiness with which the nerves had their communication completely cut off, were to me new facts, and encouraged me,

Case of fungous excrescence of the tongue.

in

in the following case of fungous excrescence from the tongue, which bled so profusely as at times to endanger the patient's life, and never allowed him to arrive at a state of tolerable health, to attempt removing the part by ligature.

which when removed was followed by violent hæmorrhage.

John Weymouth, eight years of age, was admitted into St. George's hospital, on the 24th of December, 1800, on account of a fungous excrescence on the right side of the anterior part of the tongue, which extended nearly from the outer edge to the middle line at the tip. It appeared, from the account of his relations, that the origin of this fungus existed at his birth, and had been increasing ever since. He had been a year and a half under the care of the late Mr. Cruikshank, who had removed the excrescence by ligature round its base; but, when the ligature dropped off, a violent hæmorrhage took place, and the excrescence gradually returned. Attempts were made to destroy it by caustic; but hæmorrhage always followed the separation of the sloughs; so that, after ten trials, this mode was found ineffectual. It was also removed by the knife, ten different times, but always returned.

From this history I was led to believe, that the only mode of removing the disefae was taking out the portion of the tongue upon which it grew. This was a case in which I felt myself warranted in making an attempt out of the common line of practice, to give the patient a chance of recovery; and, from the preceding case, having found that pressure on one part of the tongue produced no bad consequences on the other parts, I was led to remove the excrescence in the following manner.

The portion of tongue removed by ligature.

On the 28th of December, I made the boy hold out his tongue, and passed a crooked needle, armed with a double ligature, directly through its substance, immediately beyond the excrescence. The needle was brought out below, leaving the ligatures; one of these was tied very tight before the excrescence, the other equally so beyond it, so that a segment of the tongue was confined between these two ligatures, in which the circulation was completely stopped. The tongue was thin in its substance; and the boy complained of little pain during the operation. Thirty drops of laudanum were given to him immediately after it, and he was put to bed. He fell asleep, continued to dose the greater part of the day, and was so easy the next day as to require no particular attention.

On the fifth day from the operation, the portion of tongue came away with the ligatures, leaving a sloughy surface, which was thrown off on the 11th day, and was succeeded by a similar slough; this separated on the 15th day. The excavation after this gradually filled up; and, on the 20th day, it was completely cicatrized, leaving only a small fissure on that side of the tongue.

*(To be concluded in our next.)*

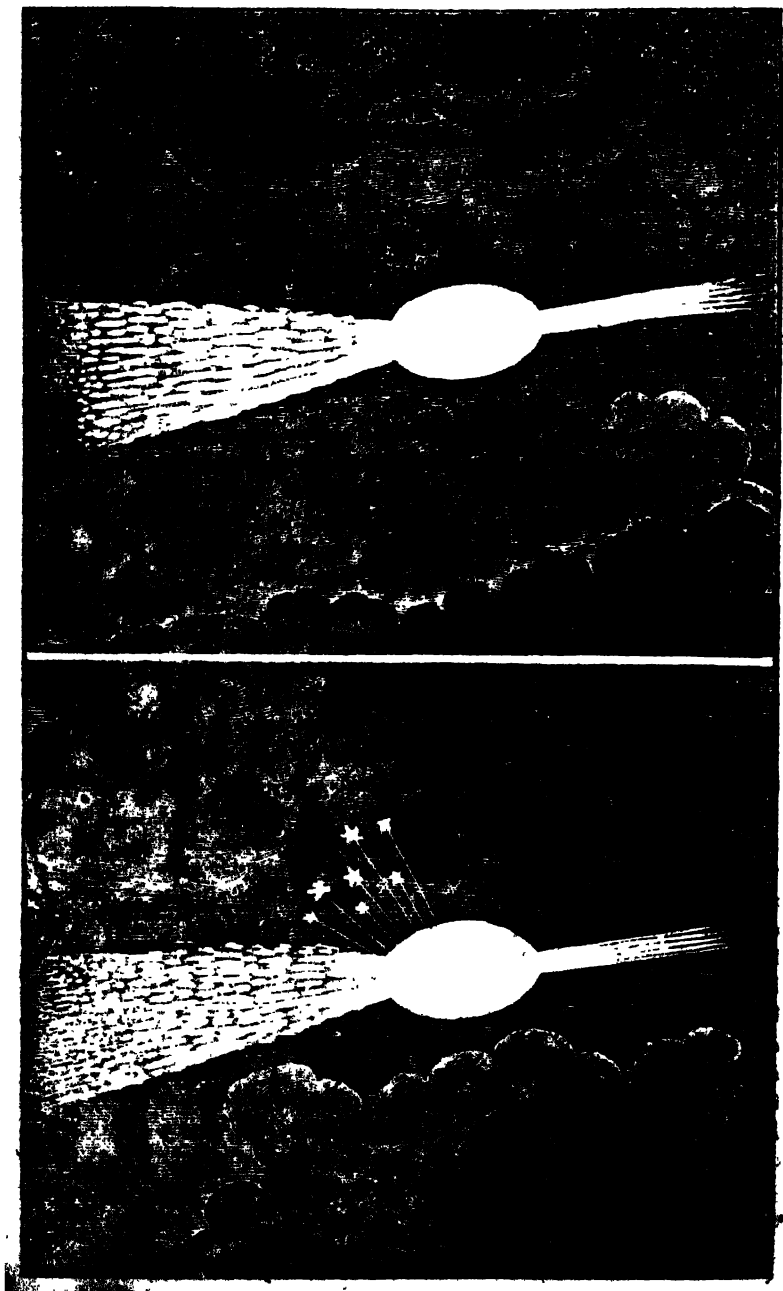
## XVI.

*Some Account of the large fiery Meteor which appeared on the sixth of last Month (November.)*

ON Sunday evening at half past eight, on the sixth of last month, I was suddenly surprized with an illumination resembling that of a flash of lightning, but more permanent. The windows of the room in which I was sitting face the south, and were not closed either by the shutters or curtains, but only by venetian blinds; through which some of the company asserted that they saw a large globe of fire moving to the westward. My back was towards the window, so that I saw only the light which appeared considerably blue, and seemed to last two, or perhaps three seconds. The blueness in all probability was not more than that of day-light, which, when contrasted with the light of candles, has a lively blue tinge.

*Account of a  
fiery meteor,*

A scientific friend of mine, who has favoured me with a sketch from which the annexed drawings were taken, was walking up Princes Street Soho, and turned upon the sudden appearance of light, when he saw the meteor passing rapidly over St. Ann's church yard, having the appearance of an oblong or elliptical solid, with a short radiating eruption from its preceding part, and numerous sparks diverging from its hinder part. He compares it to the burning of a combustible matter in oxygen, and saw it burst into many sparks, which instantly went out and left extreme darkness. Its direction seemed to be to the southward of a line supposed to cross Princes Street at right angles, which estimate would give a course about W. S. W. This gentleman saw the great meteor of August 18, 1783, which was therearound, and he thinks the present quite as large as that.



Another friend informs me, that he saw it from a station in *Account of a fiery meteor.* St. James's Park, near the Queen's House, rising above the horizon, in the east towards Westminster-abbey, and that it passed over St. James's Park, and part of the Green Park, where it was lost behind a cloud. He thinks it remained in sight for a much longer time than two or three seconds, and that it did not move in a straight line. The course by his observation would be about W. N. W.

Another person who saw it burst, speaks of the portions falling down like the sparks of a rocket.

That it, passed as most large fiery meteors seem to do, in the superior part of our atmosphere is probable from the general facts. A gentleman on Hampstead Heath, beheld the country suddenly illuminated, and clearly saw Harrow Steeple, which is eight miles distant in a strait line, and it was also seen from Dartford in Kent, until it became obscured by a cloud; but I had no further particulars respecting it.

Much difficulty must arise in estimating either the course, direction, or elevation of meteors of this kind, which appear when totally unexpected, and are gone before the mind can enter into any course of reasoning or estimate. We are fully occupied with the impressive sensation they produce, and have scarcely any other means of obtaining a conjecture respecting positions and altitude, but by repeating our observations on the spot. On this subject the reader may consult an excellent paper by Sir Charles Blagden in the LXXIV. volume of the Philosophical Transactions.

The lower sketch shews the manner of explosion into smaller pieces.



## XVII.

*A First Memoir on coloured Shadows\*.* By Cit.

J. H. HASSENFRATZ.

Shadows are generally but erroneously counted black.

WE are accustomed to consider as black, the shadow formed by an opaque body, which intercepts the light falling on a white pasteboard, although in reality, nothing is more difficult to obtain than black shadows, because it appears, that to procure a black shadow, the light must absolutely be a point, and no sort of reflected light must reach the illuminated surface.

Phenomena of shadows.

On a surface illuminated by a light which has magnitude, the shadow is always accompanied by a coloured penumbra. Whenever the illuminating body is at a greater distance than a metre from the body enlightened, and that which intercepts the light, is nearly five decimetres from this body, the penumbra and shadow have distinct colours, which are in a great measure dependent upon the nature of the combustible body which yields the light.

The blue shadow at sun-rise and sun-set, is not different from the general laws of shadows.

Since the period when Leonardo da Vinci noticed the blue shadow which is perceived in the morning at sun-rise, and in the evening at sun-set, this shadow has been attended to, and naturalists have endeavoured to explain its production, as forming a phenomenon peculiar and distinct from those of other shadows. Nevertheless, this shadow has no particularity; all the shadows which are examined are coloured, even those produced by the noon-tide sun of a clear summer's day, which are commonly called *black shadows*. The coloured shadows are produced by two or more distinct lights or by the separated parts of the same light which act differently.

Variety of coloured shadows.

Nothing is more diversified than the colours of shadows; on noticing them with attention, we remark among them all the prismatic colours: we distinguish red, orange, yellow, green, blue, violet shadows, more or less mixed with black.

The subject is to be pursued beyond this Memoir.

The object of this first Memoir, is to make known the great variety of coloured shadows which may be distinguished

\* From Journal de L'Ecole Polytechnique, Tom. IV. p. 272.

and which we have observed, and also the circumstances which give rise to them. Other memoirs which we purpose to present after this, if the institute shall think the subject worthy of its attention, will contain the series of observations we have made to ascertain the causes which produce the coloration of shadows in a great number of cases.

We shall divide this Memoir into three parts: the first will have for its object the colour of the shadows produced by the reunion of the light of the atmosphere, and that of the sun, or of the light of the atmosphere and an artificial light, but in those circumstances wherein only a single shadow is perceptible: the second will contain a description of the coloured shadows produced by the light of the atmosphere combined with reflected lights, and sometimes with the direct light of the sun: finally, the third will elucidate the coloured shadows formed on a body illuminated by two lights, natural and artificial.

#### *Part the First.*

If, near a white surface enlightened by the sun and the light of the atmosphere, a black body is placed which intercepts the solar rays, there will be seen on the plane a shadow which varies from a greenish blue to a violet black, connecting through the blue and the violet. The colour of the shadow depends on the state of the atmosphere, the latitude of the place, the meridional and northern declination of the sun, and the time elapsed between its rising and its passage to the meridian, and from the latter to its setting.

When the sky is clear, the colour of the shadow at sun-rise, at Paris, varies between blue with a slight tinge of green to violet blue. The first days of Nivose, the shadow is greenish-blue; the first days of Germinal, blue; the first days of Messidor, indigo with a violet tinge; the first days of Vendémiaire, the shadow becomes again blue to return to a greenish blue the first of Nivose.

If on a clear day, when the sun is on the equator, the variation of the colour of the shadow, if noticed from the instant of sun-rise to its passage over the meridian, it will be observed that this colour is blue at sun-rise; that at each elevation of the luminary above the horizon, the blue changes; that it becomes indigo, violet; that in the end it blackens, and that when the sun is on the meridian, the shadow is of a blackish violet.

Every

as produced by  
the meridian sun  
at different pe-  
riods of the  
year,

Every day from the first of Nivose to the first of Messidor, the solar shadow offers different tints, at the moment when this luminary passes the meridian. The first days of Nivose, the shadow is violet, a little blackish; it increases in blackness daily to the first Messidor; at this period, the shadow is violet-black.

as produced by  
the difference of la-  
titude,

If the coloured shadow of the rising sun is observed on a clear day, at the same period, and in different latitudes, it will be seen to vary from violet-blue to green, going from the equator to the pole.

The coloured shadow observed at the beginning of Nivose, at sun-rise, from Messina to Skalhott in Iceland, is, at Messina, light indigo; at Vienna and at Paris, blue, with a slight green tinge; at London, Berlin, Copenhagen, Edinburgh, a more distinct green tinge; at Petersburg, a little more green; finally at Drontheim and at Skalhott, a greenish tint.

At the same period, at noon, the shadow varies, between Messina and Skalhott, from black slightly tinged with violet to violet.

Result of the  
observations.

The comparison of the colour of the shadow of the sun, with its situation in the different places where it is observed, naturally leads to this first conclusion, that it is different in the ratio of the intensity of its light, compared with that of the atmosphere, in fact, the rising sun, on the first of Nivose, having a feeble light in comparison with that of the atmosphere, the shadow is a greenish blue; as it rises above the horizon, the intensity of its light increases, and the shadow becomes blue, indigo, violet: finally, when the sun is on the meridian, its light has acquired its greatest intensity, and the shadow blackens, preserving nevertheless a violet tinge.

Comparing in the same manner the colour of the shadow observed each day of the year at Paris, as well as that observed in each latitude on the same day, it will be seen, that it changes from green to violet-black, according to the intensity of the light acquired by the sun; and when in winter the sun, being but little elevated above the horizon, appears red, from the feeble light which penetrates the light mists existing in the air, the solar shadow is green, sometimes a fine grass green.

Experiments  
with an artificial  
light which is  
varied,

To satisfy ourselves whether the colour of the shadow depended on the relation of the light compared to that of the atmosphere, we placed the light of a lamp near a white surface, illuminated solely by the light of the atmosphere; when this lamp

lamp was five decimetres from the enlightened plane, the shadow caused by an opaque black body was blue. On bringing the lamp nearer the colour of the shadow changed successively; from blue it changed to indigo, from indigo to violet, and the violet blackened gradually. When the lamp was very close, the shadow was of a violet black colour, exactly similar to that produced by the light of the sun on a clear summer's day.

This experiment succeeds very well on days when the sun is hid by clouds, all bodies are then illuminated by the light of the sky.

A similar result may be obtained by the inverse method, with an artificial light which is fixed, that is to say, by illuminating the surface with an artificial light of constant intensity, and successively increasing the intensity of the light of the sky.

If in a dull morning, before the appearance of twilight, a white surface is illuminated by the light of a lamp placed at five decimetres distant from the enlightened surface, the shadow of an opaque black body placed at a small distance from the surface, is black very slightly violaceous. As soon as the twilight appears, the tint changes, the intensity of the violet increases. As the day brightens, the violet of the shadow effaces the black tint; at length the shadow becomes violet, indigo, and is blue when the light of the day is completely developed.

Repeated experiments with a taper or a candle, have given the same results.

It follows, from the observations detailed, that every shadow, formed on a body illuminated, at the same time, by the light of the atmosphere and the direct light of the sun; or by the light of the sky and an artificial light, such as a lamp, a taper, a candle, is coloured in all cases wherein the light of the sun, or the artificial light is intercepted by an opaque black body; and that the colour of the shadow varies from green to violet black, in the ratio of the intensity of the light of the sun, or the artificial light, compared with that of the atmosphere. But on what depends the colour of the atmosphere? what causes that variation of colour in the ratio of the comparative intensity? This is what we shall examine in another Memoir.

*(To be concluded in our next.)*

## SCIENTIFIC NEWS.

*Observations on St. John's Wort. By Cit. BAUNACH.\**

The juice of St. John's wort affords a red colour, and also a yellow.

Red most soluble.  
Mode of dyeing with it.

Various tints produced by it.

Alum and potash form the proper mordant.

Stains paper yellow,  
and dyes leather.

Contains tannin.

Sulphate of iron converts it into a concrete resin.

ST. JOHN'S wort, *hypericum perforatum*, is a resinous plant, the tops and flowers of which contain a juice soluble in water, alcohol, and vinegar. With the former two it gives a blood-red colour, with the last a fine bright crimson. When combined with [mineral?] acids or metallic solutions, it affords a yellow colour, which proves, that it contains two colouring matters, one, the red, more soluble than the other.

To dye linen, woollen, silk, or cotton yellow, it is sufficient to put them into a bath, the water of which is duly impregnated with the juice of this plant, with a certain quantity of a mordant. The best mordant for this colour is alum combined with a suitable portion of potash. The stuff must be left in the bath some time; for the durability of the colour, and the shade produced, depend chiefly on the time of continuance in the bath, the quantity of the mordant, and the degree of heat employed. When but little mordant is used, the tint is a bright yellow; by increasing it the colour is made to incline to green; and on adding a solution of tin in nitro-muriatic acid, it assumes rose, cherry, and crimson hues, all with a fine lustre. Alum alone does not answer well, the addition of potash being essential. This decomposes the alum, precipitates its earth, dissolves a considerable portion of it, and this alkaline salt with an earthy base becomes the true mordant in the process; the more because the colouring principle resides in a substance almost purely resinous.

The juice of St. John's wort, united with the mordant here mentioned gives a fine yellow colour to paper; and as it produces the same effect on leather, it may be employed with advantage for dyeing sheep and other white skins.

The plant contains a considerable quantity of tannin, as I have been convinced by means of the solution of glue, and other experiments made for this purpose.

On dropping a little solution of sulphate of iron into the juice of St. John's wort, a blackish brown precipitate is formed, which has the property of absorbing oxygen, becoming at length insoluble in water, and assuming the characters of a concrete resin.

\* From the *Annales de Chimie*.

Having distilled a certain quantity of the plant with water, the product had a powerful and agreeable smell, but I could not discover the least trace of essential oil on it.

Affords an agreeable water, but no essential oil.

The juice of St. John's wort does not dissolve either in expressed or in essential oils, but it unites very well with resins. For this purpose, the juice of the plant must be dried; which may be done very conveniently by expressing it into earthen plates, and placing these in an oven some time after the bread is drawn; it must then be powdered, and will readily combine with turpentine by rubbing them together in a brass mortar warmed. This resin, thus saturated with the juice, may be mixed with oils, either essential or expressed; and on combining it with oil of olives, the oil of St. John's wort of the shops may be formed, which, thus prepared, possesses evident virtues. If it be incorporated with linseed oil, and a small portion of oil of turpentine be added, a fine red varnish is produced, which may be advantageously employed for coating articles of furniture made of wood.

The juice does not dissolve in oils, but unites with resins. Mode of combining them.

Oil of St. John's wort.

Makes a fine red varnish for wood.

MR. RICHARD KNIGHT, who is well known to the philosophical world for the very complete Magazing of Chemical Apparatus of all kinds he has for several years past established in Foster-Lane, has favoured me with a letter, in which he very satisfactorily shews that the instrument in *Plate X.* of our last number, was not invented by Mr. Accum, but by Mr. W. H. Pepys, about three years ago, and has ever since been an article on sale in the catalogue of his warehouse. The title to Mr. Accum's paper was written by myself, as almost all the titles are; and I was led to call him the inventor from the tenor of his paper. Immediately after the publication of last number, and before I had received any letter from Mr. Knight, Mr. Accum observed to me, that he is not the inventor, and that he first saw the instrument described in a German paper. At the same time therefore that I have the pleasure to give Mr. Pepys the undisputed right to a contrivance which, in point of utility and convenience, is of considerable value, I do not see any moral error that needs correction.

## ACCOUNT OF NEW BOOKS.

*The Edinburgh New Dispensatory; containing, I. The Elements of Pharmaceutical Chemistry: II. The Materia Medica; or, the Natural, Pharmaceutical and Medical History of the different Substances employed in Medicine: III. The Pharmaceutical Preparations and Compositions; including complete and accurate Translations of the 8vo. Edition of the London Pharmacopœia, published in 1791; Dublin Pharmacopœia, published in 1794; and of the New Edition of the Edinburgh Pharmacopœia, published in 1803. Illustrated and explained in the Language, and according to the Principles of Modern Chemistry. With many new and useful Tables, and several Copper-plates, explaining the new System of Chemical Characters, and representing the most useful Pharmaceutical Apparatus. By ANDREW DUNCAN, Jun. M. D. Fellow of the Royal College of Physicians, and Royal Society of Edinburgh, and Assistant of the Linnean Society of London. 8vo, 720 Pages, and 6 Plates. Edinburgh, 1803.*

Duncan's Edinburgh New Dispensatory.

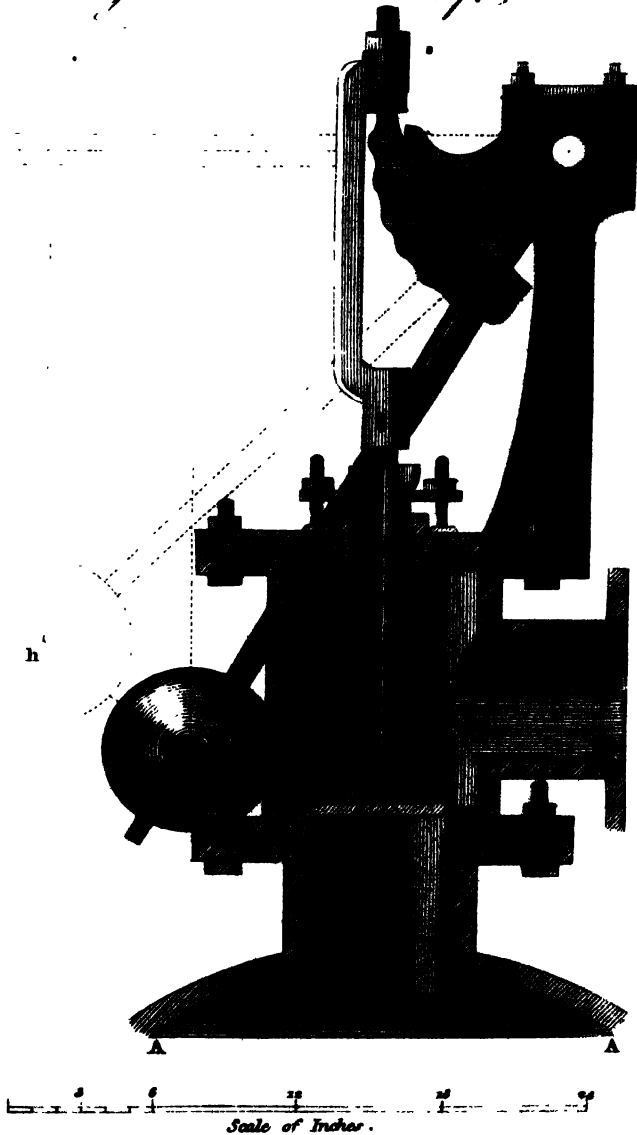
THE copious title page of this work informs the reader what he has to expect in this new edition of a stock book, the excellent foundation of which was laid by Dr. Lewis in 1753. The translation of the Dublin Pharmacopœia must be an acceptable addition, and the introductory Epitome of Modern Chemistry, a knowledge of which is indispensable to those who would understand Pharmacy as a science, or practise it with advantage as an art; was loudly called for by the many improvements made of late years in this branch of science.

It would take up too much room to give the titles of the several new tables, which are all useful: and Dr D. appears to have availed himself of every thing in the field of modern discovery, or in the best foreign Pharmacopœias, that was consistent with the plan of the work.

Accum's lectures on chemistry.

MR. ACCUM, who has resigned his gratuitous service as Assistant Chemical Operator in the Royal Institution, is about to enter on a Course of Lectures on Practical Chemistry, and its Application to Agriculture, Arts, and Manufactures. They will comprehend distinct Series of Lectures on Popular Chemistry, Operative Chemistry, Mineralogical Chemistry, Agricultural Chemistry, and Galvanism.

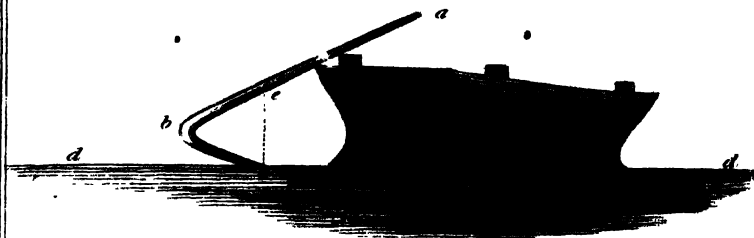
*Self acting & regulating Steam Valve.  
by M.<sup>r</sup> Arthur Woolf.*



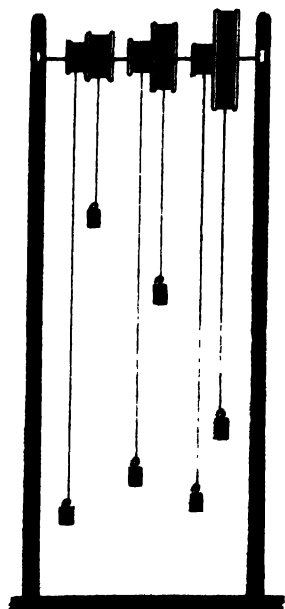




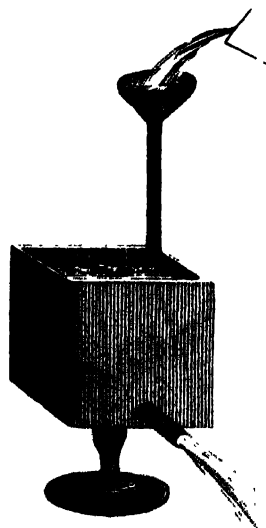
*Dr. Wollaston's  
on Terrestrial Refraction.*



*Dr. Young's Apparatus  
to Illustrate the Doctrine  
of Reponderance*

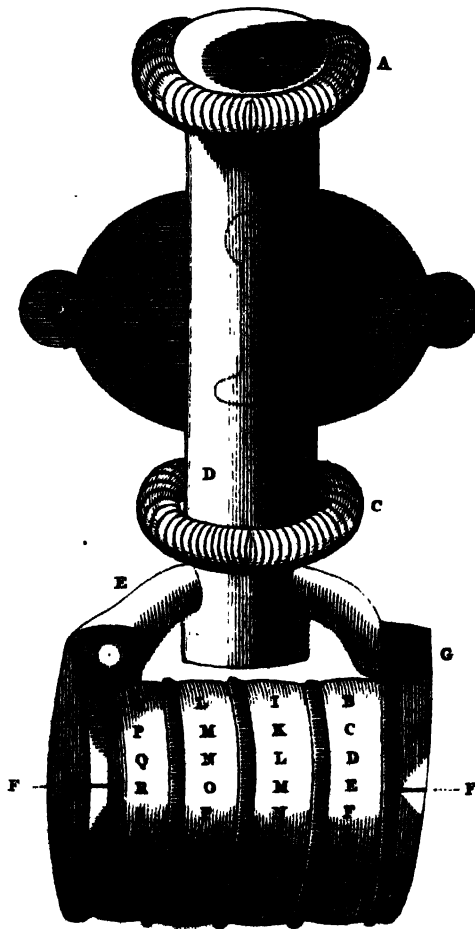


*Experiment  
of the Velocity of Water  
thro' a Vertical Pipe.*

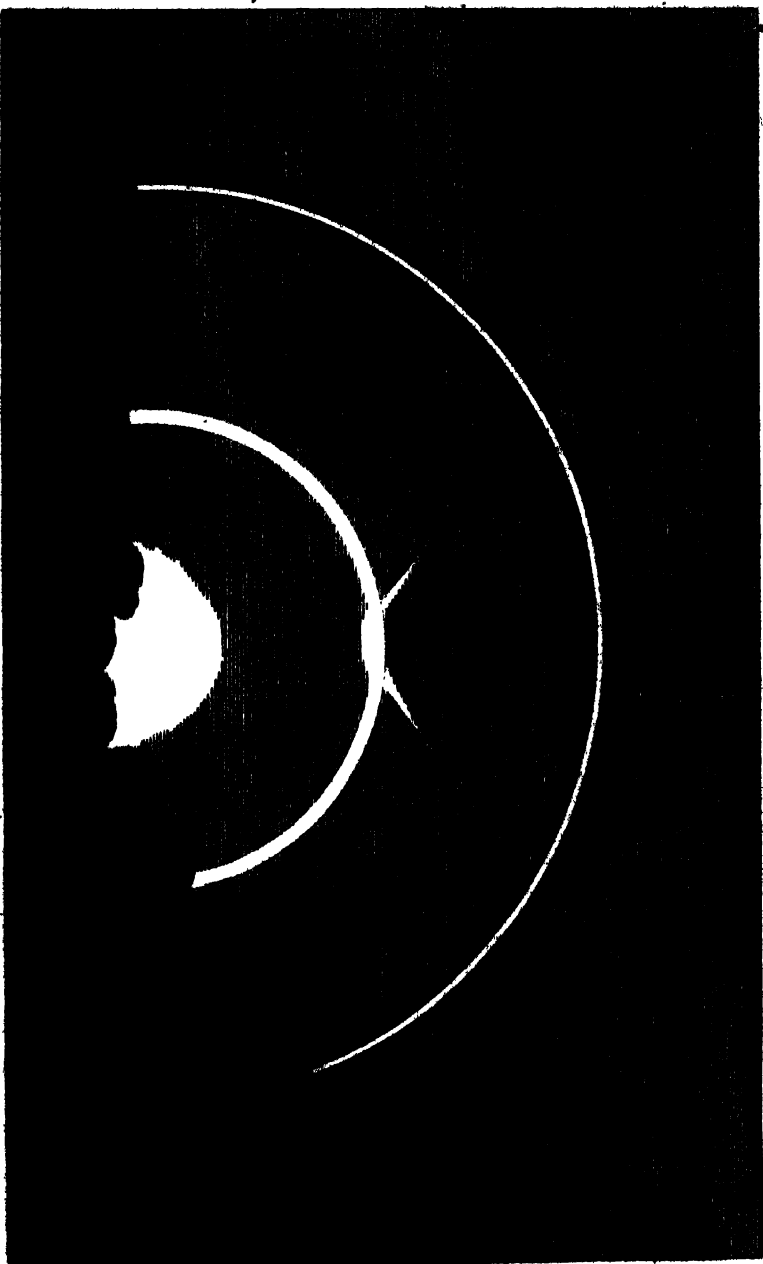




*Lock of Combination by M. Regnier.*



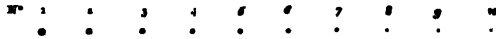






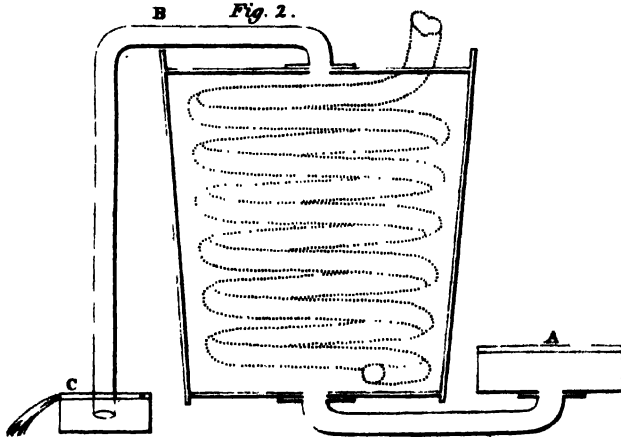
*D<sup>r</sup> Herschel on the magnified Image  
of the same Star, at different times.*

*Fig. 1.*



*Sir A. V. Edmonstone's method of easily raising  
Water in worm-tubs, condensers, &c.*

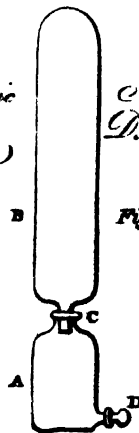
*Fig. 2.*



*Eudiometric  
By,*

*Apparatus:  
D<sup>r</sup> Hope?*

*Fig. 3.*







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